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GILBERT AND THE HISTORIANS (I) *

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I *Three Views of Gilbert's Science*

GILBERT's *De Magnete*, first published in 1600, appeared at a strategic point in the history of science, when the Renaissance era overlapped the beginnings of the scientific revolution, and its importance for an understanding of the origins of that revolution is obvious. In spite, or perhaps because, of this importance, the work has been subject to more contradictory reports and assessments than almost any other classic in the history of science.¹ Broadly speaking the views of the historians on Gilbert's scientific method fall into three classes. First there are those who, in commending his painstaking and exhaustive experimental studies, claim him as the first Baconian scientist, practising Bacon's method twenty years before the publication of the *Novum Organum*. But then it has to be admitted that there are passages in the *De Magnete*, and even more in Gilbert's posthumous work *Philosophia Nova*,² which do not fit into any simple picture of Baconian science, and so a second Gilbert takes shape: a metaphysician, Neo-Platonist, scholastic, animist,

* Received 5. vi. 59.

¹ See, for example, W. Whewell, *History of the Inductive Sciences*, 1837, 3, 45; and *Philosophy of the Inductive Sciences*, 1840, 2, 374; Park Benjamin, *Intellectual Rise in Electricity*, 1895, pp. 258-331; A. Wolf, *A History of Science, Technology and Philosophy in the XVIth and XVIIth Centuries*; A. Koyré, *Etudes Galiléennes*, 1939, pp. 100, 148, 249; and *From the Closed World to the Infinite Universe*, 1957, p. 131; G. Sarton, *Six Wings*, 1957, p. 94; G. K. Chalmers, 'The Lodestone and the Understanding of Matter in Seventeenth Century England', *Philosophy of Science*, 1937, 4, 75; E. Zilsel, 'The Origins of William Gilbert's Scientific Method', *Journal of the History of Ideas*, 1941, 2, 1; M. Boas, 'Bacon and Gilbert', *J.H.I.* 1951, 12, 466; C. Boyer, 'William Gilbert on the Rainbow', *Am. Journ. Phys.* 1952, 20, 416; D. H. D. Roller, 'Did Bacon know Gilbert's *De Magnete*?', *Isis*, 1953, 44, 10; J. Agassi, 'Koyré on the History of Cosmology', this *Journal*, 1958, 9, 234.

² Gilbert, *De Mundo Nostro Sublunari Philosophia Nova*, Amsterdam, 1651

or fuzzy speculator; 'a Renaissance figure, balanced between the modern spirit of experimentation and the medieval spirit of speculation'.¹ The apparent contradiction between these two Gilberts is commonly resolved with the remark that, fortunately, the deposit of enduring fact in his work can be separated from the ephemeral speculations, so that, to paraphrase Bacon, there remains at the bottom, all light opinions vanishing into smoke, a Fact affirmative, solid and true and well defined.

A third view is possible, however, in which the various aspects of Gilbert's science appear more integrated and intelligible. The recent reissue of the Thompson translation of *De Magnete*,² and the appearance of Popper's *Logic of Scientific Discovery* in an English edition, together provide an opportunity for reassessment of Gilbert's work in the light of a different analysis of the relation between fact and theory from that presupposed by most of his historical commentators. It is not yet possible to see in detail what effect Popper's thesis will have on commonly accepted interpretations in the history of science, but it is certain that, if taken seriously, its effect will be far-reaching, for most history of science has hitherto been written from the point of view of what Popper attacks under the name of *inductivism*. By this he refers to the beliefs that (1) there are basic descriptions of fact which can be known to be true by experience, and (2) empirical science is derived from these by means of inductive methods of generalisation and theory-construction. Part of Popper's reply³ consists in the counter-assertions that (1) every descriptive statement is itself open to question since it will always go far beyond what can be known with certainty on the basis of experience, and (2) the criterion which marks off empirical from metaphysical theories is not that empirical theories are inductively based on empirical facts, but that they are *falsifiable* by statements about conceivable empirical facts. There is no need to expand Popper's argument here, for it is well known, and what I propose to do in this paper is to illustrate these two points by considering Gilbert's work,

¹ M. Boas, op. cit., p. 467

² Gilbert, *On the Magnet, Magnetic Bodies also, and on the great magnet the earth; a new Physiology, demonstrated by many arguments and experiments*, trans. S. P. Thompson, ed. D. J. Price, Basic Books, Inc., New York, 1958. This translation was first published in 1900, and had been preceded in 1893 by a translation by P. Fleury Mottelay. Page references in brackets in the text will be to the 1958 edition. I have occasionally modified the translation in quotations.

³ K. R. Popper, *Logic of Scientific Discovery*, London, 1959, pp. 40 f, 78 f, 93 f

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and the historical interpretations of it I have mentioned, and hence to show that the third view of Gilbert which Popper's thesis suggests is more satisfactory than the traditional interpretations.

If Popper's two points are accepted as against the inductivist view, then it will follow that the inductivist historians have made their division into the two Gilberts in the wrong place. For the inductivist, the distinction will be between Gilbert the experimenter who faithfully reports what he observes and whose reports are therefore infallible and lasting, and Gilbert the speculator, who carelessly allows his theoretical preconceptions to colour his experimental reports, who illegitimately generalises to conclusions not warranted by the facts, and whose theories have therefore turned out to be false and useless and fit to be consigned, as Gilbert himself consigned those of his predecessors, 'to the moths and worms' (64). For Popper, on the other hand, the distinction will be between Gilbert the empirical scientist, who states generalisations and theories and then subjects them to stringent tests and whose experimental reports themselves *always* go beyond sense-experience and are therefore subject to further test, and Gilbert the metaphysician who suggests theories which he does not subject to tests which may lead to falsification, or which are in principle unfalsifiable. Our judgment of Gilbert's standing as an empirical scientist will clearly be modified by an acceptance of the second of these distinctions rather than the first, and a further modification will take place if we admit (as Popper himself asserts¹) that metaphysics is not wholly irrelevant to the logic of scientific discovery.

I shall try first to deal with the inductivist claim that most of Gilbert's experiments are modern in spirit and permanently acceptable *because* he did not allow theoretical conceptions to interfere with them, and secondly I shall try to assess Gilbert's theories in the light of the criterion of falsifiability.

2 Gilbert's 'new sort of Philosophising'

To you alone, true philosophizers, honest men, who seek knowledge not from books only but from things themselves, have I addressed these magnetical principles in this new sort of Philosophizing. . . . Whoso desireth to make trial of the same experiments, let him handle the substances, not negligently and carelessly, but prudently, deftly, and

¹ See for example K. R. Popper, 'The Nature of Philosophical Problems and their Roots in Science', this *Journal*, 1952, 3, 124.

in the proper way . . . for nothing hath been set down in these books which hath not been explored and many times performed and repeated amongst us [Preface to the Reader].

Thus Gilbert professes his adherence to the principle of the repeatable experiment as the foundation of natural philosophy. Widespread acceptance of this principle was undoubtedly one of the new features of the scientific revolution, and its importance is not in dispute among philosophers of science. This is not the place to discuss its historical origins, but it is interesting to remark on the self-conscious manner in which it was analysed by Gilbert and his contemporaries. For them the thesis that detailed and even tedious experiment is profitable and necessary is not a platitude but a platform, requiring all the rhetorical and polemical arts. For Bacon it is the gospel which brings salvation from the natural sins of the intellect; for Galileo, speaking of Gilbert, adherence to it requires god-like qualities:

Since we see that the ordinary human mind has so little curiosity and cares so little for rare and gentle things that no desire to learn is stirred within it by seeing and hearing these practised exquisitely by experts . . . these [Gilbert's inventions] are concepts and ideas for superhuman souls.¹

The dispute between inductivists and others does not arise at the level of the decision to rely on repeatable experiments, but at a question which is logically posterior to this, namely the question of the status of verbal reports of such experiments. The inductivist holds that such reports should, if made with proper care and open-mindedness, be statements of fact which will remain invariant to all subsequent changes in the theories designed to explain the facts. This is the presupposition of the account of Gilbert's work which distinguishes the true deposit of experiment from the froth of theoretical misinterpretation and speculation. Let us therefore look more closely at some of his experiments.

In some cases it is clear that the design of the experiments is determined by Gilbert's cosmology. There is no doubt that he himself and his immediate successors considered his major achievement to have been the demonstration that the earth is a 'great magnet'. This is apparent in the title of his book and throughout its text, as well as in the comments upon it of (among others) Kepler, Galileo, Bacon, Descartes, and Boyle. None of these positively accept Gilbert's wider

¹ Galileo, *Dialogue concerning the Two Chief World Systems*, trans. S. Drake, Berkeley & Los Angeles, 1953, p. 406

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cosmological speculations, and some reject them, but all except Bacon regard the theory of the magnetic nature of the earth as proved.¹ Gilbert's 'proof' consists of experiments on a model which reproduces the earth's form as far as possible, that is, on a spherical lodestone, or as he calls it a 'terrella'. That he should conduct most of his researches into magnetism with lodestones of this shape is therefore determined by his theory that the magnetic power and shape of the earth are closely connected, and that there is in this particular composite form—spherical magnetic substance—something elementary and fundamental in the structure of nature. The earth is the active and powerful mother in whose womb, as it were, all secondary materials grow (19), it is iron and not gold which is the most noble metal (24), iron and lodestone (which Gilbert considers to have the same form) are primary bodies, and the sphere is the most perfect form (12).

But, inductivists will argue, here is a clear case where the metaphysical froth can be blown away, leaving some undisputed facts. It was no doubt unfortunate that Gilbert chose to experiment with a magnet whose shape lessened its potential power, but still he discovered how magnetic bodies behave, and when he contented himself with bare descriptions of what he saw, his results stand. In replying to this thesis I shall try to show that if it is true, then it is never possible for the experimenter himself to know which of his reports are in this sense 'bare descriptions' and which are theory-loaded, and that what counts for the historian as a 'bare description' is dependent on subsequent theories and therefore relative to the time at which the history is written.² Thus the thesis contradicts itself, because one of its

¹ Galileo has no doubt that this is a hidden but inferred fact: the superficial appearance of the earth *conceals* a lodestone, but Gilbert's reasons 'conclusively prove *de facto* that our globe is made of lodestone'. And (Sagredo is made to say) 'I rank these experiments with the needle very little lower than mathematical proof'. (Galileo, *op. cit.*, pp. 403, 408.)

² An ironical example of how hindsight can pervert the historian's judgment is provided by Bacon's theory of heat. Most modern historians of science have little good to say of Bacon, but they have to admit that the one positive result of his method which is contained in his works, namely that heat is a mode of motion of matter, is 'correct'. Whewell, however, writing at a time when the hypothesis 'that heat consists in the vibrations of an imponderable fluid' at least included the established laws, while Bacon's included none of them, calls Bacon's hypothesis 'an example of illicit generalisation' (Whewell, *Philosophy of the Inductive Sciences*, vol. ii, p. 402). It is a measure of Whewell's sense of historical values that this does not prevent him giving a fair and just assessment of Bacon's method and achievements.

premisses is that 'bare descriptions' are invariant to change of theory. Furthermore these consequences show that it is ridiculous to base any assessments of scientific ability or personal character merely upon the fact that an experimenter reports his experiments in terms of archaic theories.

We have to show then, that what counts, for the inductivist, as a 'bare description', is always dependent upon later theories, and this will be done by showing that of various experimental reports which Gilbert would undoubtedly have regarded as careful descriptions, some turn out to be acceptable in the inductivist's sense, and some turn out in this sense to be distorted by discarded theories, and that the way in which these two classes of reports are now distinguished is dependent upon later theories, none of which could have been foreseen by Gilbert. We shall consider Gilbert's distinction between *coition* and *attraction*, his statements about *repulsion*, and his distinction between *rotation* and *coition*.

3 *Experiment and Interpretation*

The only chapter in which Gilbert discusses electric attraction is entitled 'On the Magnetic Coition, and first on the attraction of amber, or more truly, on the attaching of bodies to amber' (46). In this and the following chapter 'Opinions of others on magnetic coition, which they call attraction' (60), Gilbert carefully distinguishes between the force of *attraction* or 'allure' exerted by electrics on light bodies which are drawn to them passively, and magnetic *coition* in which bodies, whether iron or lodestone, 'run together with mutual forces'. In speaking of magnets

we say coition, not attraction. The word attraction unfortunately crept into magnetic philosophy from the ignorance of the ancients; for there seems to be force applied where there is attraction and an imperious violence dominates. But wherever we have spoken of magnetic attraction, we understand thereby magnetic coition, or a primary running together (60).

Gilbert does not overlook the fact that if a heavy magnet is in the vicinity of a light one, the latter is moved more easily and appears to be 'allured' by the former (71), and yet he describes similar phenomena in the case of a large piece of amber and light bodies in terms which appear to him to indicate a fundamental difference between the two

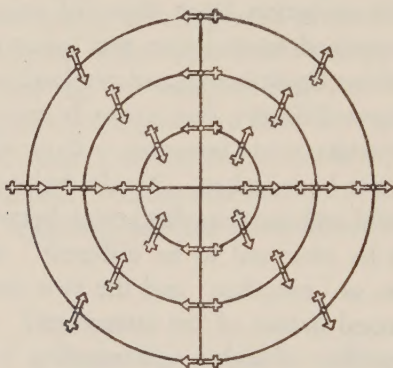
cases. It is easy to give an explanation of his distinction in terms of his own theoretical background and interests: there was at the time no conception of equality of action and reaction, and the question whether attractions were to be described in terms of one active, externally attracting body drawing another, passive, body; or of an active internal tendency of the attracted body towards the other; or of mutual attractions; was a controversial one. Furthermore, as Park Benjamin points out,¹ it was important for Gilbert to distinguish the causes of electric attraction from those of magnetism as far as possible, because he wished to maintain that lodestone and iron and their properties are alone fundamental and predominant powers in the universe. To see how Gilbert's distinction between attraction and coition fits into this pattern is not however to show that he could have known the distinction to be mistaken. No doubt he conceived himself to be drawing a careful and accurate distinction between two kinds of phenomenon which he observed to be different. And why do we regard the distinction as 'mistaken' and the two cases as identical in respect of the reciprocal nature of the attractions? Only because of our belief in the equality of action and reaction (not stated clearly until Newton), and because of much later theories about charge-induction and dielectrics.

Again, take the question of repulsion. Gilbert states positively that electrics do not repel (55, 113), although he describes experiments in which bodies move more quickly as they approach the electric and in which it is difficult to believe that he did not also observe them bounce off with considerable force, as is later reported by Cabaeus (in 1629) and by Browne (in 1646). Whatever Gilbert observed, he did not report it as repulsion, even though the possibility of repulsion would have accorded better with his theory of effluvia emanating from the electric (55). Even in the case of magnets he reports that magnetic substances are more sluggishly repelled than they are attracted (100), and describes the behaviour of two floating magnets placed with like poles together as not so much a mutual repulsion as a disposition to come into harmony:

¹ Benjamin, *op. cit.*, pp. 294 f. Although some of Benjamin's comments in his account of Gilbert are typical of what I have called inductivist history, it would be unjust not to remark here that the account is in other respects an unusually perceptive one, and that Benjamin has uncovered two significant points which have helped my own argument against inductivism. The first has just been referred to; the second is Gilbert's interpretation of repulsion (*Ibid.*, pp. 285 f).

For stone disposeth stone; the one turns the other around, reduces it to range, and brings it back to harmony with itself. When, however, they come together and are conjoined according to the order of nature, they cohere together firmly (16).

Therefore 'there is properly no such thing as magnetic antipathy' (68). It may be that Gilbert dislikes the notion of a 'repulsion' just as he dislikes 'attraction' because it savours of the occult sympathies and antipathies postulated unnecessarily in all kinds of processes which



Gilbert's experiment with a terrella and iron spikes (206).

he has himself investigated (109). But there is no reason to doubt that Gilbert is here giving a careful report of what he observes. That we should report the same phenomena in terms of repulsion is due to familiarity with a wider range of theories which lead us to treat attraction and repulsion as symmetrical.

On the other hand an example may be given of an experimental report which does turn out to accord with later theories, but where Gilbert's immediate interpretation of his report does not, and where the experiments are so difficult that the 'correctness' of the report must be said to be accidental and not anticipatory. The example concerns the cause of the earth's property of directing magnetic needles horizontally towards the north and in the vertical plane of dip. This property had previously been ascribed to the attraction of the earth for the passive needle, but Gilbert, following the English writer Robert Norman, whose *Newe Attractive* appeared in 1576, denies that the directive property is due to any attraction or coition of the poles, asserting that it is an independent disposing influence of the whole earth. Gilbert quotes two kinds of experiments in support of this conclusion. First he has himself performed experiments with a terrella and iron spikes,

in which the spikes cling on to the surface of the terrella, or if suspended above it remain at a definite angle with no tendency to be drawn directly towards the pole of the terrella. And the spikes remain at the same angle whatever the strength of the lodestone, instead of inclining more towards the pole of a stronger lodestone as would be expected if the effect were one of coition (82, 131, 195). The results of these experiments are definite, but they are not carried out in relation to the earth, and in any case the conclusion to be drawn from them is not quite unambiguous, so Gilbert refers to two experiments of a different kind which are designed to show, in the case of the earth, that when the effect of gravity is neutralised, there is no tendency of a magnetic needle to move as a whole towards the earth's poles. Both experiments are quoted from Robert Norman. Gilbert acknowledges that the first is Norman's and does not claim, by his device of marking with asterisk, that he has himself performed it and clearly demonstrated the effect reported. In the first experiment Norman magnetises an iron wire and floats it on the surface of water. It shows, he says, no tendency to move bodily, but only to rotate to point towards the north. Gilbert comments: 'This assertion of the Englishman, Robert Norman, is plausible and appears to do away with attraction' (162). In the other experiment, Norman arranges that a needle stuck through a cork floats wholly immersed in a glass bowl of water both before and after magnetisation. Again it is not found that the needle moves downwards towards the earth's pole, but only that it rotates about the cork into a dipped position. 'But', says Gilbert, 'it must be understood that as it is a curious and difficult experiment, so it does not remain long in the middle of the water but sinks at length to the bottom, when the cork has imbibed too much moisture' (204).

It must be concluded that Gilbert had tried both these experiments and failed to obtain clear evidence of the effect reported by Norman, and yet he is prepared on the basis of them to distinguish coition from the rotating property. The distinction cannot be said to be, for Gilbert, an experimentally based descriptive statement such as an inductivist would approve. And yet, as it happens, Gilbert was right in the light of the later theory according to which the earth exerts a couple but no force on a small magnet in its field. Gilbert himself however cannot approach an explanation in terms of resultant force and couple, since he lacks the notion of repulsion, and his own interpretation of the distinction between coition and rotation does not accord with the later theory. He suggests that the two forces which cause

coition and disposition to rotate have different ranges, being spread over, respectively, the 'orb of coition', which is 'all that space through which the smallest magnetic is moved by the lodestone', and the 'orb of virtue', which is 'all that space through which the virtue of any lodestone extends' (*vj).

I have selected three of Gilbert's interpretations of experiments as samples of the way in which description and interpretation are mingled, and only distinguishable in the light of subsequent theories. And this happens when, as in these cases, the experimenter is careful in his methods and precise and detailed in his reports. It may appear at first sight that inductivism is refuted by such examples, since they show that even descriptive reports of experiments are dependent on theories which may later be abandoned—they show in fact that the reports of these experiments were not 'bare' descriptions. It is never possible, however, to *refute* inductivism in this way, or by any other historical examples, because the inductivist may explain away any situation that can arise. If the theory on which an experimental report depends is still accepted, the inductivist can interpret the report as a bare description, and if it is not accepted, he can allege that no true report was given because the experimenter was blinded by his false theory and careless in his observations. Thus an argument against inductivism based upon such examples depends on the historian's judgment of the psychology of the experimenter; that is to say, it has to show that according to the historical evidence the experimenter was *not* blinded or careless, and it is never possible to show this conclusively.

Such arguments may, however, show that inductivism is unpalatable, and that there is another, more palatable, account of the historical facts which is not inductivist. This account would speak, not of Gilbert's 'facts' as distinct from his 'theories', but of those of his theories or interpretations which are now accepted in the light of further experience and those that have been refuted by it. But this latter distinction is not of great *historical* interest; it is not a distinction on which to base judgments of the scientific standing of the experimenter, for these must depend on the whole body of theory and experiment available to him in his historical situation and the use he makes of it.

Whipple Museum, Cambridge.

(To be concluded)

THE DOPPLER EFFECT AND THE FOUNDATIONS OF PHYSICS (I) ★

HERBERT DINGLE

I *Introduction*

It is doubtful if there is a serious rival to the Doppler effect as the department of modern science in which the experimental basis is slightest in comparison with the structure raised on it. Most of our knowledge of stellar motions, including the characteristics of binary stars, the evidence for the rotation of the galaxies, and the whole phenomenon known as 'the expansion of the universe' consists of deductions from this one principle. It would be fair to say that without it cosmology would scarcely exist as a scientific subject. Yet the number of cases in which we have independent evidence of a spectrum shift and an associated motion are very few, in all of them the velocities concerned are so small that they will fit a variety of formulae, and in no case at all have we evidence of the reciprocity of the effect—i.e. evidence that if A observes B's spectrum to be displaced, B will observe A's spectrum to be similarly displaced. The original justification for supposing the effect to exist was purely theoretical, and the theory concerned is no longer acceptable. This situation calls for a specially close examination of the various possibilities between which an experimental decision cannot yet be made, and so is an eminently suitable subject for the philosophy of science. It will be seen, I think, that there are problems involved in the understanding of the Doppler effect that are not generally realised, and that take us to the roots of physical theory.

2 *Discovery and Character of the Doppler Effect*

The Doppler effect is the name given to the phenomenon by which the frequency of a source of vibration appears to be increased when the source and the observer are approaching one another, and decreased

★ Received 20 v. 59

when they are receding from one another, as compared with the frequency of the same source when it is at rest with respect to the observer. In this article I am considering only the case in which the 'source of vibration' is a source of monochromatic light. The Doppler effect for sound is a far simpler, and purely physical, matter, which arouses no philosophical problems. We can observe directly that the source is vibrating and that the result is a wave motion in the material medium between the source and the observer. We can measure independently the frequency of the source and that of the waves, and verify that they are equal. We can measure independently the velocity V , the frequency n , and the wave-length l of the sound waves, and show that the equation, $V = nl$, is true. No phenomenon is known that is not perfectly conformable to these conceptions.

With light, and the source of light, on the other hand, our knowledge is almost entirely inferential. We can observe no vibration in the source, and we cannot observe light itself at all in any sense; we can make observations only on matter with which we suppose it to interact. According to current theory, the frequency which we assign to the light is not at all identical with that which we assign (partly metaphorically) to the source. The velocity of light we can measure satisfactorily only as an average velocity over a to-and-fro journey, though, with the assistance of mechanical theory, we can get a rough value for a one-way velocity from observations of Jupiter's satellites. Of the nature of light we have the most contradictory evidence. In some phenomena, such as interference and diffraction, it seems impossible to conceive of it as other than a wave motion in a medium; in others, such as the photo-electric effect, we can conceive of it only as concentrated in particles. Though we have devised mathematical formulae capable of describing both these sets of phenomena, we have not succeeded in framing a verbal description of light that will give us a uniquely clear mental picture of what it is or of how it operates. What, in these circumstances, can we say about the Doppler effect for light?

Historically, the idea that such an effect should exist was suggested by a mental picture of the nature of light (or sound) which was without ambiguity and was believed to be an accurate representation of reality.¹ The source and the observer were connected by a medium in which

¹ A brief account of the history of the subject by E. N. da C. Andrade appears in *Endeavour*, 1959, 18, 14.

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the waves travelled from the one to the other. First, suppose source and observer to be at rest in the medium. The source emits waves of wave-length λ , and frequency ν , which travel with velocity c . Since ν waves are emitted each second, and each has a length λ , the waves emitted in one second must cover a length $\nu\lambda$. If the source moves towards the observer with velocity V , still emitting waves with the same frequency, then since their velocity is a property of the medium only, the waves emitted at any instant will reach a distance c one second later. But the ν waves emitted during that second will be compressed into a distance $c - V$, since the source will have advanced a distance V beyond its initial point. The wave-length will therefore be reduced to $(c - V)/\nu$. The waves will meet the observer at the velocity c , and the number which will meet him per second will therefore be c divided

by this wave-length, i.e. $\frac{\nu}{1 - V/c}$; hence he will observe light of a greater frequency than previously. If, however, he also is moving in the same direction, with velocity v , the velocity of the waves relatively to him will be $c - v$, so the frequency of reception, ν' , will be this quantity divided by the wave-length, i.e.

$$\nu' = \nu \frac{1 - v/c}{1 - V/c} \quad (1)$$

If the motion of source or observer is in the opposite direction to that of the light, the sign of the corresponding velocity will be changed, but the formula will otherwise remain the same. We need not for our purpose consider motion of the medium with respect to some external standard, which, in the case of sound waves in a wind, may be important.

This was the formula which, until the advent of the theory of relativity, was believed accurately to represent the Doppler effect for both light and sound. It is clear that the change of frequency is not the same for a stationary source and moving observer as for a moving source and stationary observer, even when the relative motion of source and observer is the same: in the first case the frequency is $\nu(1 - v/c)$, and in the second case it is $\nu/(1 - V/c)$ and these are not the same when $V = -v$. But if $V = v$ there is no change of frequency, so that the *existence* of the effect depends only on the relative motion of source and observer, although its *magnitude* for the same non-zero relative motion varies with the individual velocities. However, in all

applications to light waves until quite recent times, v and V were always small compared with c , and since, to a first approximation,

$$\nu \left(\frac{1 - v/c}{1 - V/c} \right) = \nu \left(1 - \frac{v - V}{c} \right)$$

all that could be inferred from observation was $v - V$, and it was impossible to say anything about the individual velocities of source and observer.

3 *The Doppler Effect for a Particle Theory of Light*

Before considering the effect of relativity on these conceptions it is desirable to see what results would have been reached had a particle, instead of a wave, theory of light been envisaged, for we are now much less confident about the nature of light than were physicists in the nineteenth century. The medium, if any, between source and observer now plays no part in the matter, except to serve as a standard of rest. Clearly, if we are to speak of velocities at all, we must imply such a standard, and we may legitimately assume that it is embodied in a universal medium, or *ether*, so long as we do not grant that medium any physical properties. The frequency, ν , of monochromatic light now becomes the number of particles issuing from the source per second; the velocity of light, c , is the velocity of the particles after emission; and the quantity c/ν will clearly represent the distance between successive particles in the light train. We may represent it by λ and call it, metaphorically, the 'wave-length', since it corresponds in all kinematic respects to the wave-length of our previous considerations.

As before, then, we now suppose the source to move in the direction of the light with velocity V and to emit with the same frequency ν . But now the velocity of the light will change, for, in accordance with Galileo's principle of local motion, each particle will issue with velocity c with respect to its source, and therefore with absolute velocity $c + V$. Each particle will travel a distance $(c + V)/\nu$ before the next is emitted, but the source also will travel a distance V/ν , so that the distance λ between successive particles will be c/ν . Hence the 'wave-length' λ , will not be changed by the motion of the source. The observer, if stationary, will receive the particles at a velocity $c + V$, and the number he will receive per second will be this divided by λ , i.e. $\nu(1 + V/c)$. But if he also is moving, with velocity v , in the same direction as the light, he will receive the particles at velocity $c + V - v$, and the

number ν' which he will receive per second will therefore be

$$(c + V - v) \div c\nu = \nu \left(1 - \frac{v - V}{c} \right).$$

This, then, is our general formula for a particle theory of light.

It will be noticed that this is identical with the *approximate* formula for a wave theory, so that for small velocities the two cases are indistinguishable. For large velocities, however, this would have afforded a means of deciding between the theories, if the practical difficulties could have been overcome, but these are so great that the point is of no importance. The really important feature of the formulae is that, on the particle theory, the effect is *entirely* dependent on the relative velocities of source and observer, so that no matter what precision of measurement is attained, the Doppler effect will not, on this theory, distinguish between the motion of the source and that of the observer when they are in relative motion. We may therefore write the particle formula as

$$\nu' = \nu \left(1 - \frac{dV}{c} \right), \quad (2)$$

where dV is the velocity of the observer with respect to the source, measured in the direction of motion of the light. This, of course, makes the particle theory compatible with the principle of relativity of motion, and the wave theory, as here considered, incompatible with that principle. But the essence of the difference is not in the nature of light itself, but in the fact that, on the particle theory, the velocity of light has always the same value *with respect to the source*, however that may be moving, while on the wave theory the velocity has always the same value *with respect to the transmitting medium*. To concentrate attention on this distinction, without introducing any assumption about the nature of light, I shall henceforward speak of the 'ballistic theory' and the 'ether theory'. It must be understood, however, that these terms serve only to distinguish the two views concerning the *velocity* of light namely, that on the ballistic theory light always travels at velocity c with respect to the source, and on the ether theory at velocity c with respect to the ether—or empty space if that term is preferred—and do not necessarily imply any of the ideas associated with them at various times in the past. Also, unless otherwise stated, I shall assume that all velocities are constant throughout the whole of the time with which we are concerned.

4 *Intrinsic Properties of Light*

Let us now consider what meaning we are to attach to the three characteristics which we associate with light, namely, *velocity*, *frequency* and *wave-length*. Velocity (our standard of rest being assumed identified) is a property of the light irrespective of source and observer; for an experimenter at rest, who is unaware that source and observer exist, can time the passage of a pulse of light across a measured distance and so determine its velocity. (Strictly speaking, this is an assumption. I do not know of any experiment in which light is seen to impinge successively on two objects at a known distance apart. It is, however, universally supposed that a pulse of light is 'something' which, at least roughly, may be said to be observably located at the positions of such objects at certain times, and I shall assume that the supposition is justified.) Frequency also is a property of light itself, provided we assume, on the basis of experiment, that light is a periodic phenomenon. It is the number of particles, or wave-crests, or whatever we suppose to give a periodicity to the light, that pass a *stationary* point in unit time. Finally, 'wave-length', if it is defined as velocity divided by frequency, thus automatically becomes an intrinsic property of light, and conceptually, at least, it may be regarded as susceptible of direct determination from the wave-train by assuming a measuring rod to travel with the light. All three quantities, then, may be regarded as characteristics of the light itself, irrespective of how it is produced or received, *provided that we have an objectively defined standard of rest*. If nature does not provide one, and we can choose any one we like for ourselves, then we can vary the velocity and the frequency *ad lib.* by a suitable choice of standard, *but not the 'wave-length'*. However we vary the standard of rest, the quotient of the *velocity* and the frequency remains constant, so that, if we are to regard light as an objective entity at all, we must grant it only one intrinsic quality, namely, that which we call, literally or metaphorically, the 'wave-length'.

The postulate, or principle, of relativity (i.e. not Einstein's special *theory* of relativity, but the first of the two postulates on which that special theory is based) states that 'the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest'.¹ Light is included in such phenomena, and it

¹ A. Einstein, *Ann. d. Phys.*, 1905, **17**, 891. English translation by W. Perrett and G. B. Jeffery, London, 1923

therefore follows, if this postulate be accepted, that velocity and frequency cease to be intrinsic properties of light (unless some further qualifying postulate is introduced), and each monochromatic beam is identifiable by its 'wave-length' only. That alone remains the same when we change our arbitrary standard of rest, so that alone characterises the light itself.

Let us now look at our Doppler Effect formulae. The ether theory formula is immediately discredited, for its characteristic is that the velocity of light is always the same with respect to the medium, however it may vary with respect to the source. The medium therefore defines an absolute standard of rest, which may be identified by sending out beams of light in opposite directions from a number of bodies in relative motion. The velocities of the beams will in general be $c + V$ and $c - V$ with respect to their sources, and the source for which these quantities are equal will be that for which $V = 0$, i.e. that which is absolutely at rest. This violates the relativity principle, and the distinction has been found practically impossible since experiment gives $V = 0$ for all sources, so we need consider only the ballistic theory formula (2).

We have already noticed that this conforms to the relativity principle in that it involves dV but not V and v individually. Further, it determines ν'/ν , but not the individual values of ν' and ν . This agrees with our general considerations, but it is to be noted that ν' and ν may have definite determinable values because they are not properties of light but the *frequencies of reception* of light by the observer. If we suppose the observer at rest and source moving with velocity V , then the light travels at velocity $c + V$ and with a frequency $(c + V)/\lambda$, and if we suppose the source at rest and the observer moving, the light travels at velocity c and with a frequency c/λ . The frequency of the light itself varies with our assumption, but the frequency of reception is the same since the relative velocity of light and observer is, on either assumption, $c + V$, and the frequency of reception is therefore $(c + V)/\lambda$. When the source and observer are relatively at rest, $V = 0$, and the frequency of reception, ν , is c/λ .

To sum up, then, we see that if we postulate the existence of a periodic phenomenon called light, we can assign to it one single intrinsic property which we may call 'wave-length' (λ) and two indefinite properties, which we call its 'velocity' and 'frequency', which vary with our arbitrary choice of a standard of rest: their ratio is equal to λ , but individually they are undetermined. We can,

however, determine each of them with respect to a chosen standard of rest. In that case the velocity on the ballistic theory is always c with respect to the source and $c + dV$ with respect to the receiver, while the frequency of emission is c/λ and that of reception $(c + dV)/\lambda$.

5 *Test of the Ballistic Theory*

This is essentially the theory put forward by W. Ritz in 1908,¹ though he did not apply it to the Doppler effect. It has for long been regarded as disproved, chiefly because of a demonstration by de Sitter that any theory that requires the velocity of light in space to depend on the velocity of the source at the time of emission would demand apparent motions of the components of binary stars which do not agree with observation. de Sitter's argument is sound, on the basis of his assumptions, but it does not dispose of the ballistic theory.² The velocity of light and of the source must, on the relativity principle, be related to some standard of rest, and de Sitter tacitly chose the Earth as such a standard. But if, in accordance with the ballistic theory as here defined, we choose the emitting body, theory and observation agree perfectly. Strictly speaking, this phenomenon lies outside Ritz's considerations, for the relative velocity of the Earth and star is not constant, and its variation is an essential part of the test. But if we generalise the postulate of relativity, which Ritz accepted for uniform motions, to motion of all kinds (implying not necessarily Einstein's 'general theory of relativity', but only the postulate that any body, in whatever state of motion it may be, may be regarded as being at rest), then we would expect the velocity of light *with respect to its source* to remain constant, and not its velocity with respect to an arbitrarily chosen body such as the Earth. In that case the phenomena cited by de Sitter would be quite consistent with the ballistic theory.

A necessary implication would be that if the velocity of emission of the light were c with respect to the star, its velocity of reception at the Earth would be $c + dV$, where dV is the velocity of the star with respect to the Earth in the direction of travel of the light. The only practicable way of measuring the velocity of reception of light from a distant inaccessible source is to make use of the phenomenon of aberration. The direction in which a star appears is determined by compounding the velocity of its light with that of the orbital motion

¹ W. Ritz, *Ann d. Phys. et de Chim.*, 1908, **13**, 145

² See H. Dingle, *Mon. Not. R.A.S.*, 1959, **119**, 67

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of the Earth with respect to the Sun. Since the latter motion varies continuously in direction, the star appears to describe annually an ellipse in the sky, with characteristics that are well known and agree with theory within the limits of observational error. The stars have various motions with respect to the Earth, as is shown by their 'proper motions', and on Ritz's theory their light should reach the Earth at various velocities, and so their aberrational ellipses should differ. All such variations, however, are too small to be detected. But with the extra-galactic nebulae it may be otherwise. These show spectrum shifts which, if they are correctly identified as Doppler effects, indicate that distant nebulae are receding at velocities which should ensure quite unmistakable changes in the aberrational ellipses.

At my request, Dr I. S. Bowen, Director of the Mount Wilson and Palomar Observatories, has very kindly agreed that examination shall be made of the positions of such nebulae at different times of the year. The result is not yet available, but earlier independent observations by Strömberg, von Biesbroeck and Heckmann,¹ of a cluster of nebulae not very suitable for this purpose, show no appreciable difference between the aberrational constants for the nebulae and for stars in our galaxy. If this result is general, the conclusion is that if the spectrum shifts are, in fact, Doppler effects, and if the accepted theory of aberration of light is correct, the light from these nebulae is received at the Earth at velocity c , identical (within the limits of observational error) with the velocity of light as measured in terrestrial laboratories. There seems no reason to question the theory of aberration, but the interpretation of the spectrum shifts is much more doubtful, as we shall see. At present, therefore, we can only say that the question is open. It would be settled definitely by a laboratory comparison of the velocities of light from stationary and moving sources.

6 *Einstein's Special Relativity Theory*

A few years before Ritz's paper appeared, Einstein put forward his 'special relativity theory', which added to the postulate of relativity a second postulate, the 'postulate of constant light velocity', namely, 'that light is always propagated in empty space with a definite velocity c which is independent of the state of motion of the emitting body'.² As so stated, the postulate is equivalent to the ether theory, which we

¹ See *The Observatory*, 1959, 79, 105

² Einstein, *op. cit.*

have seen to be inconsistent with the postulate of relativity, for we have only to substitute 'the transmitting medium' for 'empty space' to make it a requirement of that theory. Kinematically these phrases are synonymous, and we are not concerned with any physical properties that might distinguish them. Einstein, however, stated that his new postulate was 'only apparently irreconcilable' with the relativity postulate. He added that 'the introduction of a "luminiferous ether" will prove to be superfluous inasmuch as the view here to be developed will not require an "absolutely stationary space" provided with special properties, nor assign a velocity-vector to a point of the empty space in which electromagnetic processes take place'.

Literally, these statements are clearly contradictory. You do not assign a velocity-vector to a point of empty space, but the velocity of light in empty space is c . Velocity with respect to what? The development of the theory shows that it means 'velocity with respect to anything at all'. You do not assign a *unique* velocity-vector to a point of empty space, but you can choose any standard of rest you like, and the velocity of light with respect to it will always be c . It is impossible to picture this, for if we have two bodies in relative motion, we cannot picture a third body having a finite velocity which is the same with respect to both. We must content ourselves with stating the result of an experimental measure of the velocity of any beam of light with respect to any body at all. The theory therefore demands that we give up the attempt to picture, not merely what it is that moves, but the process of motion itself, the passage from point to point as time goes on. That requires us to express in a formula the velocity of light with respect to two relatively moving bodies in such a way that the value c results for both.

The theory satisfies this requirement. The velocity of emission of light from any body, however it is moving, is measured as c with respect to the body—our limited experience on Earth conforms to this. At what velocity will that light be received at the Earth if the body is moving towards the Earth with velocity dV ? According to the theory the answer is

$$\frac{c + dV}{1 + \frac{cdV}{c^2}} = c.$$

Hence, no matter what value dV may have, the light will be received at velocity c . Moreover, this is not a peculiarity of light. Anything

else, moving with velocity c , would show the same phenomenon. Nor is the velocity c the only velocity that defies our attempt to picture it. If something were emitted with velocity U , whatever U might be, its velocity of reception would be

$$\frac{U + dV}{1 + \frac{UdV}{c^2}}.$$

The theory forbids us to form a picture of *any motion at all*. We can visualise the setting out and the arrival of a travelling body: what happens in the empty space between is incapable of precise expression. We can make an approximate image of it for small velocities; that is all.

Let us, then, now review our three theories with respect only to the velocities of emission and reception of light with respect to source and receiver, respectively. On the ether theory the light is emitted at velocity $c - V$ and received at velocity $c - v$. On the ballistic theory it is emitted at velocity c and received at velocity $c - dV$. On Einstein's theory it is emitted at velocity c and received at velocity c . The ether theory violates the postulate of relativity, but satisfies the postulate of constant light velocity by distinguishing the 'real' velocity of light from its measured velocities of emission and reception. The ballistic theory satisfies the postulate of relativity, but violates the postulate of constant light velocity. Einstein's special theory of relativity satisfies both postulates. The weight of experimental evidence against the ether theory (which includes, among other things, all experiments of the Michelson-Morley type) is very great. Experiment has not yet finally decided between the ballistic theory and Einstein's theory.

7 Relativity and the Doppler Effect

Now let us return to the Doppler effect, and see how our conclusions are affected by the advent of the relativity theory. The first thing to notice is that, contrary to what seems to be a general belief, there is no necessary relation at all between the relativity theory and the Doppler effect. Of course, if we have independent reasons for supposing that there is such a thing as a Doppler effect, then the relativity postulate requires that, like all other phenomena, it must not enable us to distinguish in an absolute sense between the motion of the emitter with respect to the receiver and that of the receiver with respect to the

emitter. But it would be entirely consistent with Einstein's theory if there were no Doppler effect at all—i.e. if motion had no effect on the observed frequency of light—or if the relation of frequency to velocity followed any of the infinite number of formulae satisfying the relativity postulate. The postulate of constant light velocity speaks only of the *velocity* of light: it does not require that light shall even show a periodicity. That we infer quite independently from experiment, and therefore the Doppler effect formula can have only an empirical basis, or possibly a theoretical basis in some quite independent theory.

Suppose, however, that we take light to be a periodic phenomenon, and accept the relativity postulate that nature provides no absolute standard of rest. We can then dismiss the ether theory as incompatible with the latter, and consider only the ballistic theory and Einstein's theory in an attempt to derive a *theoretical* Doppler formula. On the ballistic theory the matter is simple. The emitting body sends out something (particles or waves or anything periodic we like) at the rate of ν per second. These entities fall on the receiver at a velocity $c - dV$, so the frequency of their reception must be $\nu \frac{c - dV}{c} = \nu \left(1 - \frac{dV}{c} \right)$. This is our formula (2), which already conformed to the relativity postulate and so required no modification. If A radiates to B and B to A, each will observe the other's spectrum displaced in the same sense and by the same amount, no matter how their distance apart is changing. It remains a matter for experiment to verify that this is so and to determine whether formula (2) does in fact give the right relation. The necessary experiments have not been made, but we shall consider presently what experimental evidence we have bearing on the question.

On Einstein's theory we have no basis for assuming any formula at all. The passage of light is unpicturable, and we can form no conception of what frequency can mean when applied to the encounter of an observer with something indescribable. Nevertheless, a formula has been derived—Einstein derived it in his first paper on the subject.¹ In the simple case, which alone we need consider, in which the two bodies concerned are directly approaching or receding from one another, Einstein's formula is

$$\nu' = \nu \sqrt{\frac{1 - dV/c}{1 + dV/c}} \quad (3)$$

This clearly satisfies the relativity postulate, and, as we have seen,

¹ Einstein, op. cit.

necessarily satisfies the postulate of constant light velocity since any such formula will do that, velocity and frequency having been entirely divorced from one another.

But how, in view of such divorce, is a formula obtained? It was actually first derived by assuming the existence of light as a system of electromagnetic waves, and then applying the mathematical transformations required by the theory, whence two frequencies were obtained, related according to equation (3), and identified with observed frequencies of light from the two relatively moving bodies. But Einstein, Lorentz, and other theoretical physicists have always held that the mathematical transformations correspond to actual physical phenomena—for instance, that the change of length, L , into

$$L\sqrt{1 - V^2/c^2}$$

when the co-ordinate system is changed, represents an actual physical contraction of a rod when it changes from a state of rest in the first system to a state of rest in the second. Accordingly, if we are to relate the mathematics to actual physical events, we must express the Doppler spectrum shift in terms of physical characteristics of the source of light, the light itself, and the observer's apparatus.

But this appears to be impossible. The nearest we can get to it is the following. Let us make two assumptions. First, let us assume that light consists of a periodic structure which issues from the source with velocity c and, although meeting the observer with velocity c also, nevertheless meets him with the frequency which would be expected if the velocity were $c - dV$, as in the ballistic theory. Then, if the frequency of the emitted light is ν , that of the light reaching the observer will be $\nu' = \nu(1 - dV/c)$. The second assumption is that all periodic phenomena are such that their frequencies must be regarded as reduced, if they are moving with velocity dV , by the factor $\sqrt{1 - dV^2/c^2}$. Now the observer is moving towards the source with velocity $-dV$. Hence the frequency of the light which he emits is $\nu_0 = \nu\sqrt{1 - dV^2/c^2}$ where ν is the frequency which would have to be assigned to his light, if, like the source, he were at rest. Accordingly, the ratio of the frequency which he receives from the source to that which he emits

from his own lamp is $\nu'/\nu_0 = \sqrt{\frac{1 - dV/c}{1 + dV/c}}$

These assumptions are not only compatible with Einstein's theory; they have actually been made in the course of its development, and

they yield the expected formula. The formula has the required characteristic that it gives opposite frequency changes for approach and recession, and is a function of the relative velocity of the bodies only. But this view of the matter compels us to assume that the observer, and not the source of light, is the moving body. If the source moves and the observer remains at rest, it requires us to put

$$\nu' = \nu \left(1 - \frac{dV}{c} \right) \sqrt{1 - dV^2/c^2}$$

and $\nu_0 = \nu$, so that $\nu'/\nu_0 = \left(1 - \frac{dV}{c} \right) \sqrt{1 - dV^2/c^2}$. The two ways

of regarding the motion are thus not equivalent, and the postulate of relativity is violated. No other physical justification of the formula has ever been suggested, as far as I know, so if Einstein's theory is to be maintained we must add to the impossibility of picturing light as a moving something, a similar impossibility of picturing it as a periodic phenomenon.

In this situation the odds seem heavily in favour of the ballistic theory. Indeed, on Einstein's theory we have no reason to suppose that there is any such thing as a Doppler effect for light, even if we allow light to be periodic. It will be remembered that the evidence for the effect came originally entirely from the ether theory of light coupled with the ordinary addition law for the composition of velocities. The ballistic theory discards the ether, but still requires a Doppler effect, though with an altered formula. But Einstein's theory discards both ether and velocity addition formula, and leaves nothing to suggest that what they imply will still have significance. The natural inference, if Einstein's theory is accepted, is that the Doppler effect is a mere speculation based on an erroneous theory, and that no corresponding phenomenon is to be expected.

8 *Observational Evidence for the Doppler Effect*

This simple conclusion, however, is not now possible, for we have *observational* evidence that the effect exists. We know from sunspot observations that the Sun rotates on an axis, and the spectrum of light from opposite limbs shows displacements of the right character and roughly proportional to the respective velocities. Concordant results are obtained from the light reflected by rotating planets. There are a few binary stars, whose orbital motion is observed in the telescope,

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which show corresponding spectrum shifts. Terrestrial experiments, made with light reflected from moving mirrors give similar evidence. In all these observations the receiver is regarded as being at rest, but the orbital motion of the Earth is accompanied by oscillatory spectrum shifts in the light from stars, so we may say that the Doppler effect has been shown to exist when one body approaches or recedes from another, no matter which, on general grounds, we find it convenient to suppose is the moving one. We have no direct evidence of the *reciprocity* of the effect—i.e. simultaneous evidence that if A observes B's light to be displaced with motion, B will observe A's light to be displaced in the same sense and by the same amount. It would be highly desirable to test this, but the required observations are difficult, though perhaps not impossible in rocket experiments. In none of the observations just cited can the measurements be made precise enough to distinguish between formulae (2) and (3). In the only known astronomical phenomenon giving sufficiently large displacements—the so-called recession of the galaxies—we have no independent evidence that the galaxies are in fact receding. Indeed, we have reason to believe that in at least one other physical situation—the presence of a gravitational field—a spectrum shift is produced which is indistinguishable (so long as observations from both bodies are impossible) from the Doppler effect, although the bodies concerned may be relatively stationary. We must, therefore, as already remarked, at present suspend judgment on this point.

The most suggestive experimental evidence, however, comes from a celebrated experiment by Ives and Stilwell,¹ who observed the radiation from hydrogen atoms in a beam of canal rays which had been made to move at a high velocity by the action of an electric field. By inserting a mirror in the tube they were able to observe together, on the same plate, the radiation from rapidly approaching atoms and, in effect, from their equally rapidly receding reflections in the mirror. They accordingly obtained two spectrum lines displaced by large amounts (up to 32Å. in extreme cases), one on each side of the normal position of the same radiation. They could estimate the speed of the atoms from the characteristics of the applied electric field and the equations of electromagnetic theory, and the displacements agreed roughly with those to be expected from equations (2) and (3). From their observations they concluded that Einstein's formula (3) was the correct one.

¹ H. E. Ives and G. R. Stilwell, *J.O.S.A.*, 1938, 28, 215

The difference, however, between the formulae for each individual displacement is so small, and the normal position of the spectrum line from a stationary atom under the conditions of the experiment is so doubtful, that this conclusion must be regarded with considerable reserve. The observations, however, do afford evidence of much greater strength. Whatever the velocities may have been, it is hardly questionable that if the velocity of the approaching atoms was V , that corresponding to their reflected radiation was $-V$. Now according to formula (2) of the ballistic theory, the frequency for a stationary source is the *arithmetic* mean of the frequencies for two velocities, V and $-V$, while according to formula (3) of Einstein's theory, it is the *geometric* mean of those frequencies. Consequently, we can by-pass our uncertainty about the frequency for a stationary source under the conditions of the experiment, by seeing which mean value is the same for all velocities.

The data published by Ives and Stilwell are rather meagre, but the records of their experiments are now in the possession of Dr Pascal M. Ravier, President of the Astronomical Society of Nevada, who has kindly supplied me with a considerable selection of observations, in which the estimated values of the velocities range from 640 to 2,000 km./sec. These leave no doubt that it is the geometric mean, and not the arithmetic mean, that is independent of velocity—indeed, to a degree of accuracy that one would hardly have supposed the experimental arrangements to allow. If, then, these results are to be accepted—and, especially in view of compatible observations by G. Otting,¹ no reason appears for doubting them—we must accept it as a fact that formula (3) of Einstein's theory gives the observed relation between spectrum shift and theoretical velocity. (I say 'theoretical' velocity because it is assumed that the equations of the electromagnetic field yield velocities strictly equivalent to measured distance divided by measured time. As we shall see later, there is some reason to question this.² However, the point is unimportant here, for even if the quantity here called V is not the true velocity, the latter must be such a function, $f(V)$, of V that $f(V) = -f(-V)$, and what has been said about the arithmetic and geometric means remains unchanged.)

It appears, then, that on experimental grounds we must accept formula (3)—with the possible substitution of $f(V)$ for V —as giving

¹ G. Otting, *Phys. Zeits.*, 1939, 40, 681

² See H. Dingle, *Bull. Inst. Phys.*, 1958, 9, 314

the correct relation between velocity and spectrum shift. It is true that other formulae are possible that would satisfy the evidence—the frequency could, in fact, be any function, $\phi(V)$, of V obeying the relation $\phi(V) \times \phi(-V) = \text{constant}$ —but no other has ever been suggested, and our problem really consists in reconciling the apparent inevitability of the ballistic theory, which gives what appears to be the wrong formula, with the apparent impossibility of the Einstein theory, which gives what may be the right formula. I shall therefore assume that formula (3) agrees with observation.

The crucial experiment, as before, would be the determination of the velocity of light from a moving source on the Earth—preferably over a one-way journey, but if we are justified in assuming that light is reflected from a mirror with the velocity of incidence, a two-way journey would be satisfactory. And here it may be said that a one-way determination is by no means theoretically impossible, as is often stated. The synchronization of separated clocks by light signals presupposes nothing about the magnitude of the velocity of light: it merely defines time at a distance in such a way as to make the forward and backward journeys of the light take equal times when the clocks are relatively at rest. An experiment is perfectly conceivable in which two bodies, A and B, relatively at rest at a distance X apart, carry clocks which are synchronized by light-signals. A third body, C, moving at high velocity with respect to A and B, travels from the former to the latter, and at the instant of its coincidence with A, both A and C send out light pulses towards B. Einstein's theory is built on the assumption that these signals would reach B at the same instant: on the ballistic theory the signal from C would arrive first. The times of their arrival could be observed on the clock at B, and therefore the velocities of the two beams in the A,B rest-system could be calculated. But even without an actual measurement of the velocities, the correctness of Einstein's postulate could be tested by the fact of their simultaneous or successive arrival. It is very greatly to be hoped that such an experiment as this will be attempted if possible.

9 *The Theories Contrasted in Relation to the Doppler Effect*

In the meantime, however, let us examine the consequences for the Doppler effect of assuming, in turn, each of the possible results to be realised. First, suppose the ballistic theory to be corroborated, i.e. the light issues from the source at velocity c with respect to the source, and

reaches the receiver at velocity $c - dV$ with respect to the receiver, where dV is the velocity in the direction of the light, of the receiver with respect to the source. Then it is inescapable that the relation between the frequency of reception ν' , of the light elements (particles, wave-crests, . . .), and the relative velocity, dV of source and observer is given by (2), viz. $\nu' = \nu(1 - dV/c)$. On the other hand, the Ives-Stilwell experiment requires us to conclude that the relation between the spectrum frequency, N' , and the relative velocity of source and observer is $N' = N \sqrt{(1 - dV/c)/(1 + dV/c)}$. Here by 'spectrum frequency' I mean the quantity, commonly called the frequency of the light, which is derived from spectrum measurements, and N is the spectrum frequency when source and observer are relatively at rest.

It follows that $\frac{N'}{N} = \frac{\nu'}{\nu} \frac{1}{\sqrt{1 - dV^2/c^2}}$. This is consistent with the general relation

$$N = \frac{A\nu}{\sqrt{1 - dV^2/c^2}}, \quad (4)$$

where A is a constant, and the primes have been omitted for convenience, N, ν corresponding to a relative velocity dV between source and observer.

This leads us to inquire into the physical meaning of the quantity which we derive from the spectrum and call the 'frequency' of the light. It is usually, perhaps too naively, supposed to be identical with ν , and in a passage in section 7 we have implied this identification. But a little reflection shows that the matter is much more complicated than that. Let us take the simplest—and at the same time the most fundamental—evidence of the existence of a spectrum frequency. Consider monochromatic light incident normally on a Fabry-Perot interferometer, which consists of two parallel, half-silvered plane glass plates separated by a small empty space. The light that emerges on the other side consists of a number of beams whose paths of travel in the instrument differ successively by equal finite lengths. One beam goes straight through; another is reflected to and fro once before transmission; another twice, and so on. If the distance between the plates is e , the paths are therefore $e, 3e, 5e, \dots$. What is observed, with a certain arrangement of the apparatus, is a number of concentric narrow rings of light on a dark background. If, now, we gradually change the distance between the plates, all the rings expand at the same rate

and others appear to open out from the centre, preserving the general pattern. When the distance has changed by a certain small amount, de , the original pattern is exactly restored, each ring having moved out into the place previously occupied by its neighbour. A change by $2de$, $3de$, . . . restores it again, so we have evidence of a *periodicity* in the phenomenon, the period being related in some way to the distance de —which corresponds, of course, to a difference $2de$ between the paths of neighbouring beams in the set that combine to give the pattern.

We have been accustomed to explaining this by supposing that the light-train has a structure that repeats itself every $2de$ units of distance, and we call $2de$ the *wave-length* of the light and represent it by λ . If we change to light of a different colour, $2de$ has a different value, and so we say that beams of light of different colours have different wave-lengths.

But clearly this will not do. If there are two such interferometers side by side, receiving light from the same source, they of course yield the same 'wave-length'. If, now, one moves towards the source, the light is not changed, as observations with the other will prove, but we obtain a different value of de with the moving one. Accordingly, de cannot measure a property of the light, and we know that λ is a property of the light.

The same thing is seen, perhaps even more compellingly, by considering two sources of different kinds of light, at rest side by side; to fix our ideas, suppose they emit the hydrogen red line at $\lambda 6563\text{\AA}$. and the lithium red line at $\lambda 6708\text{\AA}$. When our interferometer is stationary we obtain a certain value for de with the hydrogen line. When it moves towards the sources, a particular velocity is reached at which we get this same value of de for the lithium line. *All* the indications of our interferometer, and indeed of any kind of spectroscope at all, will be exactly the same for the light from the two sources under these conditions. Hence, quite apart from all theories, de cannot measure the wave-length of light, however 'wave-length' be interpreted.

But if the spectrum does not measure a property of the light, what does it measure? It can only be some characteristic of the interaction of the light with the apparatus, which changes with their relative motion. Let us, then, put $2de = f(\lambda, dV)$, where λ is a property of the light alone and dV is a property of the source-receiver system alone. If N be the spectrum frequency, then, by definition, $2de = c/N$, so that

$f(\lambda, dV) = c/N$. Now we have seen that the Doppler formula (3) requires, on the ballistic theory, that $N = \frac{A\nu}{\sqrt{1 - dV^2/c^2}}$, so that $2de$ on that theory must be $\frac{c\sqrt{1 - dV^2/c^2}}{A\nu}$. The wave-length λ , is, as we have

seen, a property of the light, independent of its relation to source or receiver, and is equal to $(c - dV)/\nu$, i.e. the ratio of the velocity and frequency of reception. Hence

$$A \cdot 2de = \lambda \sqrt{\frac{1 + dV/c}{1 - dV/c}} \quad (5)$$

I do not know of any means by which this relation can be tested, since we have no practicable means of measuring λ ; all our methods of measuring what we call the wave-length of light measure de , and not λ , and may give, as we have seen, the same value for kinds of light which we know have different 'wave-lengths', whatever 'wave-length' might mean. Nor have we sufficient knowledge of the structure of light and the process by which it interacts with the interferometer to derive a theoretical relation between de , λ and dV . We can only say that if formula (3) is correct, then on the ballistic theory this relation must hold.

Now let us turn to Einstein's theory, and see what interpretation it gives of the change in de when the interferometer changes from a state of rest to a state of approach with respect to the source of light. The light itself does not change, so λ does not change. By hypothesis, the relative velocity of the light and the interferometer does not change. Hence the frequency of reception of the light, which is the quotient of velocity of reception and wave-length, does not change. But de does change. There is only one possibility: the dimensions of the apparatus must change with motion, so that an adjustment must be made to restore the original pattern. But if this be the explanation, the apparatus must expand when moving in one direction and contract when moving in the other, for de changes in opposite senses in the two cases, and this is contrary to the general requirements of Einstein's theory concerning the effect of motion on the size of a body. We must accordingly reject this supposition, and admit the phenomenon to be inexplicable.

We are once more thrown back on the complete unpicturability of the physical processes occurring in these experiments, and compelled to acknowledge the theory as a calculating device, predicting what we

shall observe in given circumstances, but yielding only contradictory ideas if we try to form a physical conception of what is taking place. We are familiar with this kind of situation in quantum theory, but there the entities whose location or motion is inapprehensible are themselves purely conceptual; we have not previously encountered such an anomaly in macroscopic physics. But this does not necessarily condemn Einstein's theory. In the last resort, a theory fulfils its purpose if it does in fact agree with observation at all points; it need not have anything to say about what happens between the observations. At present both the ballistic theory and Einstein's theory satisfy this basic requirement, so far as they have been tested, and although on aesthetic grounds the former is the more attractive, a decision can be made between them only by such an experiment as that already mentioned or else by the detection of inconsistencies in one of them. We shall return to this point.

(To be concluded.)

SIMPLICITY IN SCIENTIFIC THEORIES ★

H. R. POST

THE term simplicity is often invoked in connection with scientific theories.¹ Yet there are few technical discussions of it.

The merit of a scientific theory is judged not only by its logical consistency, and its correspondence with experience. The absence of logical contradictions and the absence of empirical evidence contradicting statements of the theory are not the only criteria for acceptance of the theory. (Indeed many crank theories, precisely the ones most difficult to eliminate, would qualify for acceptance under these two criteria.) There are, of course, grave problems attaching to these first two criteria. But the remaining ground on which we judge a theory is certainly less well defined still. There is, even now, a widespread feeling that it could not, or should not, be more closely defined, that it is essentially an aesthetic criterion of 'elegance' or 'simplicity', and that such a criterion is ultimately subjective in application. This is not the view of this paper. The establishing of objective standards in this field is not only of academic interest. It might well prove to be of practical value.

We shall refer to the remaining criterion of merit of a theory as

★ Apart from minor changes, this is a paper read at the Philosophy of Science Conference, Oxford, September 1958.

¹ ' . . . it will always be possible thus to reformulate a law, postulated at first only for certain co-ordinate systems, such that the new formulation becomes formally universally covariant. Beyond this it is clear from the beginning that an infinitely large number of field-laws can be formulated which have this property of covariance. The eminent heuristic significance of the general principles of relativity lies in the fact that it leads us to search for those systems of equations which are *in their general covariant* formulation the *simplest ones possible*; among these we shall have to look for the field equations of physical space.' (Albert Einstein, 'Autobiographical Notes', *The Philosophy of Albert Einstein*, Evanston, 1949 p. 69.)

' . . . for the prediction of an unknown relation between physical quantities, the common method of dimensional analysis has no logical or mathematical basis. If we employ it in the hope that it may lead to useful results, this hope must be based not upon any established principles of science, but upon some vague belief in the simplicity of nature.' (G. N. Lewis, 'Ultimate Rational Units', *Philosophical Magazine*, 1925, 49, 750.)

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'simplicity'.¹ This is without prejudice to the possibility that several, more or less independent, criteria may be involved. In fact, we shall find two closely linked criteria ('linguistic' and 'semantic simplicity') and one criterion ('pragmatic simplicity') that is, at least logically, independent of the other two.

It should not be necessary to emphasise the *importance* of clarifying the notion of 'simplicity'. The issue arises in education, where ease of understanding due to familiarity on the basis of particular previous school training is confused with technical simplicity inherent in one theory rather than another. The confusion is analogous to the confusion of 'obvious' in mathematics in (a) the psychological and (b) the technical sense.

There is a more ambitious use to which, one might hope, a well-defined measure-of-simplicity might ultimately be put: the elimination of certain classes of crank theories or, more generally, the relative grading of theories according to merit.

Most ambitious of all: a clarification of the notion of simplicity might be of heuristic value in constructing new theories.

Finally, there is the old question whether simplicity in science is entirely a tautological result of scientific activity, or whether the universe is simple in some objective sense. This question is answered here by reference to one type of simplicity.

There is, in some cases, some measure of agreement in the judgment of simplicity between different judges: We feel one theory to be unnecessarily complicated, clumsy, perhaps an *ad-hoc* theory, while another theory is more elegant in covering the same field.

In spite of our conviction that one theory is simpler than another, we nevertheless immediately run into a difficulty.

The most obvious approach to measuring the simplicity of a theory would be to base our judgment of simplicity solely on the linguistic form of the theory. But this will clearly not do: the linguistically simplest possible statement is $X \equiv 0$, and *any* theory can be cast into

¹ The choice of name implies, of course, the view that there is considerable overlap between the notion of simplicity in common usage, and the 'third criterion' discussed here. As we shall see, it covers such, perhaps at first sight diverse, but actually linked, ideas as 'brevity of statement', 'strength', 'falsifiability', and 'fruitfulness' of a theory.

Criteria of simplicity, logical consistency, and agreement with experience, are put forward as jointly necessary and sufficient in judging a theory.

this form by suitably loading the definition of X .¹ On the other hand, I claim that the question of the linguistic form of a theory is far from being trivial, either: two scientific statements may cover precisely the same range of facts, have the same empirical content, and yet may differ significantly in merit as theories or building stones of theories.

To give some examples²: (1) Consider the finite number of lines of the hydrogen spectrum known at the time of Balmer's discovery. We might compile a list of the frequencies. Compare this with Balmer's statement of the *same* frequencies:

$$\begin{aligned} R(1/2^2 - 1/3^2) \\ R(1/2^2 - 1/4^2) \\ R(1/2^2 - 1/5^2) \\ R\ 1/2^2 \end{aligned}$$

Even if Balmer's law did not imply an infinity of lines, I claim that it already presents a simplification in the representation of the finite number of lines.

Incidentally, there are cases where the number of data is *essentially* finite, such as the 'radii' of the planetary orbits. Bode's law is a simplification over a mere list of the radii.

(2) Nor need the data be given with infinite (or even *present*) experimental accuracy by the simple law. It was shown that, while Prout's hypothesis is not strictly true, nevertheless the random chance of atomic weights being as near to whole numbers as they are is only one in a hundred. Thus Prout's hypothesis is a worth-while simple, though approximate, statement over the finite domain of known elements.

(3) Usually simplification does mean reduction to whole numbers. Nevertheless there are examples of simplification in the continuum:

¹ We might, for instance, define X as $\text{curl } \mathbf{H} - 4\pi\mathbf{j}/c$, thus reducing Maxwell's equation for the case of steady currents to the form $X \equiv 0$. Again we might spuriously 'unify' two field theories (such as the theory of electro-magnetism on the one hand, and of gravitation on the other), by defining a new field with correspondingly more components. As long as these components are completely separate, this is, of course, a trivial linguistic dodge. These examples are only intended to point up the necessity for a criterion less arbitrarily based on a particular language. (I differ from Popper, *The Logic of Scientific Discovery*, London, 1959, p. 143, in considering that we must explicitly guard against classes of what are there called 'irrelevant' co-ordinate transformations.)

² Examples are taken from the physical sciences; the biological and psychological sciences add special problems not all of which are relevant to the discussion at this stage.

SIMPLICITY IN SCIENTIFIC THEORIES

Considering the solar system only, there is no empirical difference between Copernicus's system and Tycho Brahe's last system. Nevertheless the heliocentric system undoubtedly represents a simplification linguistically, by a convenient choice of co-ordinate system.

This kind of simplification makes the information at the same time more easily remembered.¹ The simpler statement is easier to remember. 'A good theory is a good mnemonic'.

These examples show that there is already some significance in ordering theories according to their simplicity entirely in the linguistic field, introducing the notion of Linguistic Simplicity, Simplicity I. This type of simplicity will be related in this paper to the better-established Simplicity in the semantic field, Simplicity II, Semantic Simplicity.

This could be described as the *power* of a theory. We identify at least two types:

(a) Two theories may differ in generality: one theory may state conservation of momentum for elastic collisions, another, simpler one, may state conservation of momentum for any system. The latter specifies fewer conditions to be fulfilled.

Now here we have one type of simplicity, at any rate, that we can point out as an 'objective' feature of nature: we may formally describe any scientific law as the statement of a correlation between some parameters under certain conditions.² Thus there is a correlation between an electric current (measured electrochemically) in a wire and a magnetic field produced by it in the air surrounding it. Now one example of the strength of the law expressing the proportionality between current and field is that it holds whatever metal may be used for the wire. 'The chemistry of the wire is an irrelevant parameter'. This fact is not a matter of convention. The usefulness of a law depends on the vast range of parameters irrelevant to it.

Many features of our universe can be described (within certain limits and to a certain accuracy) by laws that are simple in this sense. This is not a matter of convention (we might imagine a completely chaotic universe in which no laws would be possible, though we could

¹ This remark is intended to point out a parallelism between simplicity (as exemplified above) and suitability for memorising, *not* to offer a psychological definition of simplicity.

² It seems that argument arises even on this point. It would lead too far to do more than state my view that this covers predictive laws which I do not consider essentially distinct from laws expressed in other forms.

hardly exist in such a universe). We summarise this aspect of simplicity in the statement:—*the irrelevance of the values of some parameters to the invariant correlation of some other parameters is not a matter of convention*. The more parameters can be ignored, the greater the simplicity of the law.

(b) We may also grade with respect to simplicity laws showing different dependence on a given parameter: a linear relationship does appear to be simpler than one depending on the second power, etc. Circular orbits for planets are simpler than elliptical ones.

Popper¹ has shown how his criterion of falsifiability covers both types (a) and (b) of (what is here called) semantic simplicity. A theory is the simpler, the more easily it could be falsified. The degree of falsifiability depends on the number of parameters that have to be fixed in order to make it logically possible for the theory to be falsified. The simpler theory is richer, more powerful, it says more.

If the reader agrees with the preceding qualitative discussion of what we mean by simplicity, we can now proceed to outline the principles which allow a measurement of simplicity. The first step is to establish a relation between Simplicity II and Simplicity I. We shall show that they are complementary: we may think of a theory as carrying a certain message, having a certain semantic content. Two theories may differ in semantic content. If they disagree, empirical tests may decide between them, though we may assign relative simplicities to the two theories even before then. Suppose, however, we consider two theories (having the same range of application) at such a stage that they do not contradict each other: one theory may state that planetary orbits are elliptical, while the other, simpler, one states that they are circular. The latter is simpler because four position determinations could falsify it, but not, in general, the former. We may, however, graft *ad-hoc* additions to the poorer theory so as to make it semantically equivalent to the richer, simpler, theory. For instance, in the case of elliptical orbits, we may specify the ratio of major to minor axis length to be unity.²

By such ad-hoc additions we can reduce the discussion of simplicity to a linguistic one, semantic and linguistic differences being alternative aspects in comparing two theories.

¹ Popper, op. cit., Chapter 7

² A similar procedure applies in case (a): we may consider the poorer theory to allow a very general functional dependence on certain parameters, while the stronger, simpler theory specifies the function to be a constant.

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In order to compare the linguistic simplicity of two theories carrying the same message we just measure the length of the message in the two cases. For this we need a common basis-language, which raises a problem we will consider in a moment.

But given a basis-language, any theory can be regarded as a code message, presenting a message in the basis-language in a highly condensed form by a system of coding. Two semantically equivalent theories are obtained from the same message in the basis-language, by different systems of coding. *The problem of finding the simplest theory is the problem of optimum coding.* The procedure of coding in this sense is discussed in a paper on memory mechanisms.¹ The author considers the problem of economy in storage of information. The procedure is as follows: Any repeated pattern in the original message is noted. If it occurs sufficiently frequently, it is worth while to give a code symbol to that pattern, and to re-cast the message into the coded form, together with a statement of the code back into the original language. Repeated coding may be indicated.

This is essentially a procedure in constructing theories: We note recurrence in overall change, we find invariants and give them a name. There are no emergency baptisms in science. We only give a name to a quantity if we think it is going to survive, to stay invariant under a range of conditions, to be conserved, such as energy, charge, momentum.

Thus I think that the rule I tentatively stated at the beginning: '*A good theory is a good mnemonic*' may be seen to be founded on an isomorphism between theory-construction and a certain type of memorising.

Since the code itself adds length to the message, there is an optimum coding beyond which further coding would increase the total length of the message. The ' $X \equiv o$ ' formulation is shown up to be extremely lengthy allowing for the statement of coding.

I have, without apology, implied an original message of finite length to express the semantic content of either theory. This may be appropriate in special cases; in most cases it is unknown whether the universe of discourse is finite or infinite, but we may imagine that we are starting with a common basis message at such a theoretical level that the message is already linguistically finite in length.

¹ R. C. Oldfield, 'Memory Mechanisms and the Theory of Schemata', *Brit. J. of Psychology* (General Section), 1954, 45 14

I now come to the vexed question of basis-*language*. We must, of course, prevent admission of alternative basis-languages that would result in different ordering of theories according to simplicity. It is necessary, and possibly sufficient, that the basis-language should distinguish verbally between discernibles (i.e. between situations that are distinguishable either by direct observation, or as the result of experimental investigation). Such a requirement is, I think, in any case reasonable.

The need for some restriction on the choice of basis-language is apparent from the following: We could, of course, always reduce a curve to a straight line and at the same time change a straight line into a curve by an appropriate change of co-ordinates, thus inverting the order of simplicity.

But our co-ordinates are ultimately defined operationally in physics, e.g. in classical mechanics by the translation of a rigid rod, and using the same length of rod as unit along a co-ordinate axis constitutes a preferred co-ordinate system, as long as our language is capable of distinguishing between rods of different lengths. Thus we do have scales of co-ordinates that are preferred for physical reasons, being more directly related to certain operations of measurement. Similarly there are 'natural' parameters to be used, though closer inspection will show that their choice, too, is based on some form of empirical correlation. Thus the straight line which might be defined in mechanics with reference to the motion of massive bodies, may be correlated with the straight line in optics defined by a light ray.

Simplicity frequently amounts to expressing a law in terms of the natural numbers. The preference for natural numbers *might* be considered a convention, but coding into the natural numbers has the advantage that by defining the successor operation we can simplify further by re-coding.

G. N. Lewis's claim that physical laws, expressed in his 'ultimate rational units', are likely to involve coefficients made up of small natural numbers and multiples of π is, then, a claim that simplicity will show up linguistically on coding into a preferred system of units specified in advance. Similarly, Einstein's statement quoted at the beginning of this paper, amounts to a demand for linguistic simplicity in the covariant formulation.

Finally, there is another criterion of merit for a theory, its fertility. I shall refer to it as Simplicity III, *pragmatic simplicity*. Again, there is an interesting link between Simplicity III and Simplicity II. In this

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case it is not a logical relation, but the existence of a connection is a matter of experience in science (see the remark on the absence of 'blind alley' theories below).

The merit of a theory lies not only in its logical content, but in its suggestive power. We might consider the strict logical implications of a theory as its dividends. In that case good theories generally yield a bonus as well.

Thus Maxwell's theory predicted the phenomenon of displacement current as a logical implication. But Maxwell's equations also exhibited Lorentz-invariance, and were the starting point for Einstein's special theory of relativity, which substituted Lorentz- for Galileo-invariance throughout physics. This was suggested, but not implied, by Maxwell's theory.

Then there is the general experience that a theory reveals aspects of reality beyond those explicitly stated in the theory's most rigorous formulation. Molecules postulated on chemical grounds can practically be 'seen' these days: Kekulé's benzene ring model was a theory within formal chemical valency theory. But images of such rings have now been seen projected directly on to the screen of a field emission microscope. Particles postulated to explain gas pressure can now be counted. A molecule 'long' according to its structural formula exhibits mechanical properties of blocking the beaker it is in. Such extensions of experience may well exceed the expectation of the authors of the original theory.

Bridgman¹ points out that the existence of two or more independent operational definitions of a concept may be considered to justify referring to the concept as 'real'. We have referred to such 'natural parameters' under the heading of 'semantic simplicity'. Schlesinger² makes the further point in his exposition of Bridgman's views, that we should expect still further manifestations of such a 'real' quantity. That is one statement illustrating 'pragmatic simplicity'. Again, Maxwell's³ distinction between physical and mathematical models brings out the suggestive power of the physical features of a model, pragmatic simplicity, as distinct from the mathematical model designed to show up formal isomorphism between different theories.

¹ P. W. Bridgman, 'The Nature of Some of our Physical Concepts', this *Journal*, 1951, 1, 257-73

² G. Schlesinger, 'Bridgman's Operational Analysis', this *Journal*, 1959, 9, 306

³ E.g. Maxwell 'On Faraday's Lines of Force', *Collected Papers*, Cambridge, 1880

The question whether a theory is 'worth-while', rather than doubt concerning its internal consistency or agreement with experience, arises particularly in the case of 'intermediate level' theories, such as in chemistry¹ and in the theory of solids, subjects which are now accepted to be ultimately embedded in fundamental physics. It is interesting that there seems to be hardly any instance of a theory that is simple at its own level and turns out to be a 'blind alley' theory in the sense of not leading into the larger theory.²

Pragmatic simplicity is clearly related to the question of a heuristic procedure for *extending* theories. (All types of simplicity have some bearing on the construction of theories.) It is still called *simplicity* because it is to be measured by the *specificity* of the implied extension. The implication is not a strict logical one, but the theory is the better the more *specific* the implied extension is.³

The merit of Dirac's theory is not that it allowed *some* other particles. Its semantic simplicity lies in its relativistic invariance, and in its inclusion of spin. Its pragmatic simplicity lay in the fact that it might have been used to predict a specific particle with *unique* invariant charge and rest-mass, while *retaining* its semantic simplicity as specified above.

It would lead too far to consider the general question of heuristic procedure in constructing new theories. I do not share a popular view likening a scientist searching for an adequate theory to a machine scanning *at random* within the given field. Indeed, within a rather wide theoretical framework certain general considerations relating to uniqueness, invariance (of which symmetry is a familiar example), and continuity, are constantly applied by theoretical physicists, in constructing new theories of elementary particles. The first two heuristic principles are closely connected with maximising simplicity as defined above.

Certainly such criteria are applied *within some* theoretical framework. And Heisenberg's remark, *a propos* his new theory, that the universe

¹ Lectures on organic chemistry can be a rich source of *ad-hoc* theories of valency.

² This point will be taken up and discussed in greater detail elsewhere.

³ Throughout this paper, the view has been that the better (simpler) theory is more specific in its predictions (case (b)), or *requires* less specific conditions than the poorer theory (case (a)). The opposite view seems to be expressed by Kapp ('Ockham's Razor and the Unification of Physical Science' this *Journal*, 1958, 8, 265-81). His 'principle of minimum assumption' is falsified, for instance, by the validity of universal constants. Even if these values should only be valid locally in space-time, the whole point of a scientific theory is that it does *restrict* possibilities.

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has turned out to be not the best possible, but at any rate the simplest possible, has to be interpreted in this sense. *If we start with a framework of relativity and require that certain known facts be fitted, the elementary particle masses may come out correctly as unique values in a theory that is the simplest amongst those satisfying these requirements.* No universe containing as complex a creature as Professor Heisenberg could possibly be called the simplest possible. On the other hand, granted the diversity of phenomena to be explained, it is striking how simple the theories are that do such a good job.¹

To sum up: simplicity in scientific theories can be defined (Semantic Simplicity). The known universe is then found to be simple in two respects: it is, at least to some extent, describable by simple laws, and these laws contain suggestions how they may in turn be fitted into a wider context of still simpler laws (Pragmatic Simplicity). To make measurement of simplicity possible in principle, two points are made here: One, significant simplification is already possible on the purely linguistic level, and two, there is an essential connection between this Linguistic Simplicity and Semantic Simplicity. The procedure of simplification on the linguistic level is a system of coding (this is suggestive for a future discussion of heuristics and, incidentally, shows an analogy to memory processes). Starting with a basis-language preferred on operational grounds, an inverse measure of linguistic simplicity is provided by the length of the coded message stating the theory.

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¹ To give an example of applying simplicity criteria to work still developing: The wave-mechanical representation of a many-identical-particle system in first quantisation is clearly inept. The quantum-mechanical many-body problem is simpler than the classical one precisely because of the non-individuality of particles. The problem is still unsolved. Nature, however, provides us with consistently simple special instances of solutions ('shell structure'). It is likely that the explanation lies not in postulating special *ad-hoc* mixtures of interactions, but in the general analytic solution of the many-identical-particle problem.

ON PARTIAL IDENTITY OF CAUSE AND EFFECT

PAUL G. MORRISON

I

It appears to be an essential feature of most views of efficient causation that a causative event both begins sooner than its effect does and ends after the effect begins. And from such a standpoint, it would obviously be inconsistent to regard *all* of some causative event as part of its immediate effect. On the other hand, descriptions of certain common biological processes appear susceptible to an interpretation allowing for cases in which *most* of a cause is construed as part of its effect.

The aim of this essay is to explore such an interpretation, as well as an alternative view which accommodates the same descriptive subject matter, but which rules out calling one event the cause of another immediately resulting from it whenever the two have even a single space-time part in common.¹

An example from the physiology of animal motion will illustrate a causal process which might be construed according to either of these interpretations. On the contemporary view, the physiological cause of a response or vital action by a higher animal is the rapid breakdown of an energy-releasing phosphorus compound, such as adenosine triphosphate (ATP), which has been formed and stored earlier in the animal's muscle tissue. After the organism has been stimulated appropriately, each muscle to be used in the response is excited in such a way that some of its own store of ATP is broken down chemically, liberating energy for the contraction of the muscle. More generally, a certain chemical change inside the muscle tissue causes, or results in, its contraction. Now this latter statement deserves close attention.

¹ From his declaration that the efficient cause is always contiguous to its effect, it appears that Hume regarded cause and effect as physically nonoverlapping events. See David Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge, Oxford, 1896, Book I, Part III.

More recently, the four versions of the principle of causality given by Carnap also appear implicitly to rule out cases in which any part of a causative event might be construed as part of its immediate effect. See Rudolf Carnap, *Introduction to Symbolic Logic and its Applications*, New York, 1958, p. 212.

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For as will be shown presently, alternative referents for the expressions 'a certain chemical change' and '(muscular) contraction' can be selected in each instance of the process, so that if the word 'causes' is understood in one way, most of the cause will be identical with part of the effect, while if 'causes' is understood in the more customary manner, the cause will be *discrete* from (have no parts in common with) the effect. On the latter interpretation, however, the description of the process may become quite complicated in a different way to be brought out below.

2

To see more clearly what is involved in these alternate interpretations, let us first consider a model instance, B, of the physiological process in question, as represented in the accompanying diagram (Fig. 1). B is an episode of the career of a piece of muscle plus whatever foreign matter may be lodged in it. During the first two-thirds of the episode, the chemical breakdown of ATP inside it gives rise to a contractile motion of B to the right. During the remainder of the episode, the muscle relaxes, returning quickly to its original position. For the breakdown of ATP is completed, and hence the energy required to increase or maintain the contraction is no longer forthcoming.

All of these features of this greatly oversimplified model are evident in the diagram, which represents episode B as a sequence of fifteen *states* of equal duration, each indicated by a row of letters bearing a label ('B₁', 'B₂', . . . 'B₁₅') showing its place in the sequence. B may also be divided exhaustively into nine non-overlapping *portions*, concurrent with one another and with the whole of B. Hence any state of B will be a composite of one state each from each of the nine portions of B. The five odd-numbered portions, represented in the diagram by the five vertical strings of the letter 'X', are pieces of muscle fibre. The four even-numbered portions are non-fibrous components of B which start out in state B₁ as equal amounts of ATP, and which are successively broken down into waste and mechanical energy in B's later states. These even-numbered portions of B are represented in the diagram by four staggered inner columns, each consisting of a string of impressions of one of the first letters of the alphabet.

The breakdown of ATP in any non-fibrous portion of B is represented in the corresponding column of the diagram by the appearance

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of a capital letter in one row, and of the corresponding lower-case letter one row higher. Thus, in the second column of the diagram, we find a 'C' in the bottom row, and a 'c' in each higher row, indicating that in the passage from the first to the second state of episode

<i>States</i>	<i>Portions</i>	<i>of</i>	<i>B</i>
<i>of B</i>	1 2 3 4 5 6 7 8 9		
B ₁₅	X c X d X e X f X		
B ₁₄	X c X d X e X f X		
B ₁₃	X c X d X e X f X		
B ₁₂	X c X d X e X f X		
B ₁₁	X c X d X e X f X		
B ₁₀	X c X d X e X f X		
B ₉	X c X d X e X f X		
B ₈	X c X d X e X / f / X		
B ₇	X c X d X e X / F / X		
B ₆	X c X d X / e / X F X		
B ₅	X c X d X / E / X F X		
B ₄	X c X / d / X E X F X		
B ₃	X c X / D / X E X F X		
B ₂	X c X D X E X F X		
B ₁	X C X D X E X F X		

FIG. 1

B, the ATP in the second portion of B was broken down into waste which then persisted through the remaining thirteen states of that portion.

The steady contractile displacement of B and of each of its portions to the right in states B₂ through B₁₀ is indicated by the offset of each of the corresponding rows in the diagram to the right of the row next

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below it. The relaxation of B—its rapid return to the left in states B₁₁ through B₁₄—is symbolised by the sharper displacement to the left of the corresponding upper rows.

3

Let us consider some causal aspects of this complex of events, speaking first of the increment of contraction resulting from the chemical breakdown of some one non-fibrous portion of B. In the passage from any one of B's first four odd-numbered states to its even-numbered successor, the ATP in the odd-numbered state of one of the non-fibrous portions of B is broken down chemically into waste in the next even-numbered state of that portion, and the mechanical energy released during the breakdown is expended in B's next two states in moving the whole of B uniformly to the right. Thus, the breakdown of the ATP in the second portion of B is represented by the appearance of a large 'C' in the second position of the bottom row in the diagram, and the occurrence of a small 'c' in the next higher row. The consequent motion of the whole of B to the right, resulting from the chemical breakdown Cc, is represented by showing the row standing for the state B₃ one place further to the right than the row standing for B₂, and the row representing B₄ one place further to the right than that representing B₃.

We may now distinguish more clearly the two sorts of causal interpretation mentioned above. On the first of these, the cause of the contraction is taken as the scattered individual consisting of four mutually isolated parts of B—the chemical reactions Cc, Dd, Ee, and Ff—while the contraction itself is the individual made up of the unbroken sequence of states from B₂ through B₁₀. Since it was postulated that all states of B have the same duration and that each non-fibrous portion of B began with the same amount of ATP, it follows that three-fourths of the chemical breakdown is a physical part of the resulting contraction in B. For the last three increments of chemical change—Dd, Ee, and Ff—are parts of the contractile motion, since each of them is an instance, not only of the breakdown of ATP, but also of part of a displacement to the right caused by a previous breakdown of ATP. Moreover, the model presented here has been oversimplified to the extreme. For in describing an actual instance of muscular contraction, it would be necessary to postulate a good many more than four increments of chemical change and contractile motion.

And consequently, considerably more than three-fourths (but never all) of the chemical breakdown in the muscle would coincide physically with part of the movement resulting from it.

On the second interpretation, for which causes are to be construed as physically discrete from their effects in every case, it will be necessary to look upon the chemical breakdown in B, not as a single scattered causative event, but rather as four different causes—the chemical reactions Cc, Dd, Ee, and Ff. And similarly, instead of taking the contraction in B as a single effect, we must construe it as four different effects—the increments of contraction B₃₋₄, B₅₋₆, B₇₋₈, and B₉₋₁₀. In short, to avoid taking the whole contractile process in B as one in which the cause overlaps the effect, we must look upon the whole process as a set of four lesser causal processes so chosen that the causative part of each is discrete from its proper effect. For in the example, chemical change Cc results in contractile motion B₃₋₄ without overlapping it; chemical reaction Dd leads to motion B₅₋₆ without overlap; and so on. Once again, however, in explaining any actual case of muscular contraction, we should have to divide the overall event conceptually into a great many more than four—perhaps four thousand, perhaps forty thousand—causal events in order to keep cause and effect from overlapping. Hence it would seem that the simplicity of concept inherent in construing cause and effect as occurring always in discrete places must be paid for in special cases like the one we have been considering by replacing a single causative event, x , with a myriad of lesser causes, $\gamma_1, \gamma_2, \dots, \gamma_n$, and dealing with the summation of the latter as a class, f , of which they are the members.

On the other hand, still another course is open to those who want to maintain a concept of non-overlapping cause and effect without being forced to consider entities like the total chemical change and the overall contraction in B as classes of their increments. For while refusing to speak of the whole chemical breakdown in B as *causing* the muscular contraction which it overlaps, and while refusing to speak of the overall contraction, conversely, as an *effect* of the chemical change, we may make a terminological decision to call the contraction a *result*, even though not an effect, of the chemical breakdown. A result, in this sense, would amount to a totality of effects—a composite event having those effects as its physical parts, rather than a class having them as its instances. And in the frequent cases in which that totality of effects does not overlap the totality of their causes, the result would also be an effect.

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In fact, construing cause and effect as asymmetric converse relations between physically discrete events occurring at different times, we may define 'x results from y' in that general sense as follows: *x RESULTS from y, when there are two equinumerous classes of events, f and g, such that each event in g causes exactly one event in f, and such that x is the individual sum of f, and y is the individual sum of g.*¹ By this definition, x and y are physical individuals. For each is the individual sum of a class, just as New England is the individual sum of a certain class whose members are states in the north-eastern part of the United States. Moreover, the definition accommodates our earlier example. For no provision in it requires the overall chemical breakdown to be discrete from its result, the muscular contraction in B. And finally, the decision to distinguish result from effect would have the important consequence that every effect of an event would also be a result of it, while only those results which were discrete from the events producing them would also be effects of them.

4

As formal exploration of the conceptual framework of the biological and behavioural sciences continues, it is quite likely that a number of other causal processes commonly studied in those disciplines will be found to exhibit the kind of physical overlap spoken of here. In anticipation of that development, this paper was written mainly to show a natural pattern of events which calls for a modification, one way or another, of the conceptual apparatus now used in describing causal processes. Perhaps this modification should take a form like that of our first interpretation, involving a decision to generalise the reference of the words 'cause and effect' to cover situations with the kind of physical overlap found in episode B above. Or perhaps we should adopt an interpretation like the second one, choosing a different pair of expressions, such as 'producer and result', to carry this more general meaning, while adhering to the older use of the words 'cause

¹ This definition is broader than our physiological example would require. For it leaves open the possibility that some members of a causally related group of events are both effects of earlier events in the group and causes of later ones. This additional scope was provided to accommodate those cases of reciprocity involved in the important phenomenon of *feedback*. For even though feedback involves an interchange of causal rôles between two individuals, it can always be described in terms of a uniform temporal process—in terms of an asymmetric causal progression from one non-recurrent state or stage of the two individual careers in question to a later one.

and effect' to cover the very considerable subclass of cases involving no overlap of the kind found in B.

At present, it is not clear which of these alternatives is preferable. But in working toward a progressively more uniform general terminology for the language of science, it is often desirable to bring out general inadequacies in the language currently used to describe a given subject matter and to suggest tentatively various ways of circumventing them, without also pressing immediately for the adoption of some one alternative by everyone who deals with that subject matter.

5

Many philosophers would dismiss the views just expressed, on the grounds that causal terminology is outmoded in the language of science. They would maintain that, since the advent of the Uncertainty Principle in microphysics, causal formulations of empirical regularities have been shown to be inferior in principle to statistical ones, and that, in any case, empirical science deals only with probability connections between features of the world, while causal statements unrealistically assert necessary connections.

In answer to this view, we may admit that necessary causal connection is an outmoded concept in empirical science. On the other hand, adopting the viewpoint implicit in the cybernetic concept of a Markovian machine, we may usefully replace the older notion of necessary effect by the more general concept of probable effect.¹

Another group of philosophers would call upon disciplines like theoretical physics for indications of the superfluity of causal generalisations in science. They would point out that the theoretical scientist has long been replacing causal statements by quantitative ones which correlate magnitudes—such as **force**, mass, and acceleration, for example, in the mechanical law ' $f = kma$ '. And since, in any specific instance of its application, ' f ', ' m ', and ' a ' refer to values obtained through simultaneous measurements, this law is obviously not formulated in terms of efficient causation. These philosophers would argue, therefore, that the proper aim of the physiologist in the case considered above would be to specify a functional law correlating increments of chemical change with increments of contractile motion, rather than to speak in an outmoded way of a causal connection between the chemical change and the muscular contraction.

¹ Cf. W. Ross Ashby, *An Introduction to Cybernetics*, New York, 1956, pp. 225 ff.

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The difficulty with this position is that the magnitudes to be correlated in the physiological example are not simultaneous ones. For in each instance, they characterise the temporally staggered components of a common process. In each case, the release of chemical energy starts before the muscular contraction does, since it takes a short time for the released energy to pass on to the contractile proteins which are to move the muscle. Hence, unlike the above-mentioned mechanical law, any version of this physiological law which might correlate the relevant magnitudes in non-causal language would, nevertheless, be equivalent to various possible causal formulations, however cumbersome, in ordinary textbook prose.

More generally, when we turn from the equations of theoretical physics to systematic exposition in electronics, physiology, or cybernetics, we discover a fundamental concern for cause-and-effect relations in the probability sense. For investigators in the latter disciplines are looking for laws which describe repeatable patterns of change within local systems with known persistent structures. And when these repeatable processes are divided into stages or states, they find it indispensable to refer to certain features of later states as *effects* of earlier features of those processes.¹ In fact, such basic cybernetic concepts as feedback, channel, and stability appear to be inseparable from a causal concept of some kind.

To sum up, while it appears that causal terminology has lost its usefulness in the more highly formalised parts of sciences like theoretical physics, it still appears to play a vigorous and useful rôle in those parts of science which are more directly concerned with the control of process in our technical and everyday pursuits.

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¹ Cf. Ashby, *op. cit.*; especially his definitions of 'immediate effect' and 'ultimate effect' on pages 56 and 57.

DISCUSSIONS

MORE ABOUT LORENTZ TRANSFORMATION EQUATIONS

WHEN an author and his critic make peace it would be rash for a third person to comment, but this note is only in amplification of what Professors Grünbaum and Popper had to say in the Discussions appearing in this *Journal*; ¹ it is more about what was not said than about what was.

Popper ² remarked that the FitzGerald-Lorentz (F.L.) contraction hypothesis mainly and merely explained the null result of the Michelson-Morley (M.M.) experiment without making additional testable predictions. Grünbaum ³ commented that the F.L. contraction hypothesis is testable in the case of the Kennedy-Thorndike (K.T.) experiment and that in this case the hypothesis makes a falsifiable prediction to the effect that a positive shift can be observed. He added that Lorentz invoked a further *ad hoc* hypothesis of time-dilatation to make the experimental evidence available at his time conform to the expectations of the aether-theory. He did not explicitly say so but seemed to imply, and rightly, that the linear contraction and time-dilatation hypotheses could explain the K.T. experiment. Popper agreed approvingly, adding that although the F.L. hypothesis was an advance in the sense that it made a testable, but an erroneous prediction, it was more *ad hoc* than special relativity and talked of degrees of 'adhocness' related inversely to degrees of testability.

The first point we want to make is that the time-dilatation concept, although it chronologically followed the suggestion by FitzGerald of a linear contraction, was, with Larmor ⁴ complementary to the latter and preceded Einstein's theory of special relativity (S.R.). Lest Larmor's *Aether and Matter* should not be easily accessible, we quote Whittaker. ⁵

Larmor, *Aether and Matter* (1900), in commenting on the FitzGerald contraction had recognised that clocks as well as rods are affected by motion: a clock moving with velocity v , relative to the aether *must* run slower, in the ratio

$$\sqrt{1 - \frac{v^2}{c^2}} : 1$$

The second point is that with Lorentz ⁶ the equations for contraction and dilatation were not just *ad hoc* assumptions to explain only this or that experiment but followed

¹ This *Journal*, 1959, 10, 48-50

² K. R. Popper, *The Logic of Scientific Discovery*, 1958, p. 83

³ There is a minor mis-statement in Grünbaum's comment which refers to the horizontal and vertical arms of the interferometer in the M.M. experiment, etc. In fact, both the arms lay in a horizontal plane.

⁴ Larmor, *Aether and Matter*, Cambridge, 1900; see, for example, p. 174

⁵ E. T. Whittaker, *History of the Theories of Aether and Electricity*, II, p. 32 n.

⁶ Lorentz, *Proc. Amst. Acad.* (English edition), 1903, 6, 809. See also p. 31, Whittaker, *op. cit.*

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from the equations for transforming electrodynamic equations from a frame at rest relative to the aether to another frame in motion relative to the first. We are not maintaining that Lorentz's transformation equations of 1903 meant the same thing as Einstein's¹ of 1905, although both have exactly the same algebraic form. Lorentz indeed, assumed an absolute time and an absolute standard of length associated with a frame at rest in the aether. At a conference² on the M.M. experiment held at Mt. Wilson on the 4th and 5th February 1927, where Michelson, Miller and Kennedy were present, Lorentz said :

A transformation of the time was also necessary. So, I introduced the conception of local time which is different for different systems of reference which are in motion relative to each other. But I never thought that this had anything to do with real time. This real time for me was still represented by the older classical notion of an absolute time, which is independent of any reference to special frames of coordinates. There existed for me only one true time. I considered my time-transformation only as a heuristic working hypothesis. So, *the theory of relativity is really solely Einstein's work.*³

Asked if I consider this contraction as a real one, I should answer 'yes'. It is as real as anything we can observe.

Thirdly, it appears to us that L.T.E. in the context they were derived by Einstein lead to logical difficulties. In the present note we shall deal with one such difficulty. But the same equations according to Lorentz's ideas can be consistently interpreted and applied. This is possible because the physical bases in the two cases are different. It is, of course, true that the implications of L.T.E. were not fully worked out by Lorentz before Einstein. In particular, the extension of L.T.E. to all physical phenomena and not merely to electrodynamics is due to Einstein. But even Einstein's special theory took years for development, and all the predictions and consequences of the theory were, and could not have been worked out, in the first paper of Einstein (1905). But the transformation equations with Lorentz as well as Einstein are the same algebraically.

Let us first quickly derive L.T.E. according to the theory of relativity. This is necessary to make sure that the arguments can be developed internally in this note to the extent it is possible to do so. We use a method commonly employed.

S' is a frame of reference moving with velocity v relative to frame S in the direction of x and x' axes. The two frames are equivalent for the description of the laws of nature, in particular the velocity of light in all the directions is c in both the frames. Consider a light signal originating at the instant the origins of the systems S and S' coincide. The spherical front of the expanding light-wave in the two frames is

¹ Einstein, *Electrodynamics of Moving Bodies, The Principle of Relativity*, New York, 1923

² *Astrophys. Journal*, 1928, 68, 385-388

³ At the Jubilee of Relativity Theory held in Berne in July 1955, Max Born in his address entitled 'Physics and Relativity', provoked by Whittaker's opinion that the special theory originated with Poincaré and Lorentz, laboured the point regarding priority in the discovery of relativity theory, but he had apparently not come across this statement of Lorentz. A remark of Lorentz to the effect 'To discuss Einstein's Principle of Relativity here, etc.' quoted by Born is weaker in assertion than the statement of Lorentz quoted above.

given by $x^2 + y^2 + z^2 = c^2 t^2$ and $x'^2 + y'^2 + z'^2 = c^2 t'^2$. Assuming $y' = y$ and $z' = z$, we have

$$x^2 - c^2 t^2 = x'^2 - c^2 t'^2 = 0.$$

We see by substitution that the following transformation equations hold good :

$$\begin{aligned} x' &= x \cos \phi + ict \sin \phi & i &= \sqrt{-1} \\ ict' &= ict \cos \phi - x \sin \phi, \end{aligned}$$

in which ϕ has to be identified.

Where $x' = 0$, $x = -ict \tan \phi$. Now the origin O' of S' ($x' = 0$) will have in S a variable co-ordinate $x = vt$. We may,¹ therefore, put $vt = -ict \tan \phi$, thus giving

$$x' = (x - vt) / \sqrt{1 - \frac{v^2}{c^2}}, \quad y' = y, \quad z' = z,$$

and

$$t' = (t - vx) / \sqrt{1 - \frac{v^2}{c^2}}.$$

These are L.T.E. Although they were derived only for an optical case, they were assumed by Einstein to be applicable to all physical phenomena. We shall now apply them to transform, a sphere of light-wave propagated in the frame S , from this frame S into the frame S' ; in particular, we shall find the *space geometry* (x' , y' , z') of the wave-front in S' at time t' .

In S , the intercepts on the three axes are :

$$x = r, y = r \text{ and } z = r,$$

at time $t = r/c$.

Transformed to S' the corresponding observed intercepts on the three axes are :

$$\begin{aligned} x' &= (x - vt) / \sqrt{1 - \frac{v^2}{c^2}} = (r - vr/c) / \sqrt{1 - \frac{v^2}{c^2}} = r \left(\frac{c - v}{c + v} \right)^{1/2} \\ y' &= r \text{ and } z' = r \end{aligned}$$

$$\text{at time } t' = \left(t - \frac{vx}{c^2} \right) / \sqrt{1 - \frac{v^2}{c^2}} = \left(\frac{r}{c} - \frac{vr}{c^2} \right) / \sqrt{1 - \frac{v^2}{c^2}} = \frac{r}{c} \left(\frac{c - v}{c + v} \right)^{1/2}.$$

The expanding sphere (r, r, r) in S is thus transformed into an ellipsoid

$$\left[r \left(\frac{c - v}{c + v} \right)^{1/2}, r, r \right] \text{ in } S'.$$

¹ There are infinitely many relations which satisfy the equations

$$x^2 - c^2 t^2 = x'^2 - c^2 t'^2 = 0.$$

Even the number of linear solutions is infinite, e.g.

$$\begin{aligned} x' &= \left[x - \psi(\mu) t \right] / \left[1 - \frac{\psi^2(\mu)}{c^2} \right]^{\frac{1}{2}} \\ t' &= \left[t - \frac{\psi(\mu) x}{c^2} \right] / \left[1 - \frac{\psi^2(\mu)}{c^2} \right]^{\frac{1}{2}} \end{aligned}$$

whatever μ may be physically, $\psi(\mu)$ being any function of μ whatever.

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The simple calculations given above also show that the velocity of propagation along the y' and z' axes is not c but $c \left(\frac{c+v}{c-v} \right)^{1/2}$.

Has there been a mistake? We have used L.T.E. for transforming from the frame S to the frame S' and that is what the transformation equations are for. Einstein, in his first paper of 1905, while dealing with the transformation of light-rays (not the shape of material bodies) also said :¹

The spherical surface—viewed in the moving system—is an ellipsoidal surface. . . .

But a little earlier in the same paper (p. 46) he stated:

The wave under consideration is therefore no less a spherical wave with velocity of propagation c when viewed in the moving system. . . .

The principle of relativity demands that a spherical wave-front of light be observed as spherical in the 'moving' system, but L.T.E. do transform it into an ellipsoid because L.T.E. leave the y and z co-ordinates unaffected but not the x co-ordinate. The time co-ordinate is transformed homogeneously (i.e. equally for all the three space co-ordinates) giving a velocity of propagation along the y' and z' axes greater than c .

L.T.E. no doubt transform $x^2 + y^2 + z^2 - c^2 t^2$ into $x'^2 + y'^2 + z'^2 - c^2 t'^2$, but when we consider the transformations *separately for space and time co-ordinates*, we notice that the two frames S and S' are *not* equivalent. When $x^2 + y^2 + z^2 - c^2 t^2$ is transformed by L.T.E. into itself, v , the relative velocity disappears, but v enters into the transformation equations of space and time co-ordinates taken separately. Now, in any experiment, space and time measurements usually are, or, at any rate, can legitimately be made separately and the 'equivalence' of two systems in a 4-dimensional continuum is hardly enough to secure 'equality' in all measurable physical respects.

There is thus a basic antagonism between L.T.E., which discriminate between two frames in relative motion, and the principle of relativity which does not. The notorious clock-paradox also arises from this antagonism.

Various subterfuges are employed to explain L.T.E. consistently with the principle of relativity. The contraction, etc., are called 'apparent', or it is said that there is only one observer in frame S etc., etc. It will be surely noticed, however, that (x, y, z, t) and (x', y', z', t') are the co-ordinates *assigned and measured in the two frames without stating a point of preference for one or the other and without any specification regarding the observer or the mode of observation*.

It is often asserted that the transformations are reflexively true, and that this fact establishes the equivalence of the two frames—a reflexivity which is established merely by changing signs and superscripts. The question, physically, is simply this: when we go from one inertial frame to another in uniform motion relative to the first, how do we transform the co-ordinates from the first to the second? In particular, do the contraction and dilatation of S.R. obtain? Three answers are usually given: the question is meaningless; yes, the contraction, etc. exist, but they cannot be detected; no, because all inertial frames are equivalent, or, since the standard of rest can be

¹ Einstein, *Electrodynamics of Moving Bodies, The Principle of Relativity*, New York, 1923, p. 57

arbitrarily chosen and contraction, etc. can thus be arbitrarily ordered, they do not occur. Since the first view-point is less known, we are quoting Sommerfeld,¹ one of its proponents.

The frequently raised question whether the Lorentz contraction is 'real' or 'apparent' is, of course, just as idle as the question whether a body 'actually' moves. The distinction between a moving system and a system at rest, which we permitted in the preceding (development of L.T.E., etc.) for the sake of simplicity of expression, is equally meaningless and arbitrary.

Before we deal with the interpretation of experimental evidence, we mention a formulation of the principle of relativity in terms of what Whittaker² called a postulate of impotence, to the effect: 'It is impossible to detect a uniform translatory motion, which is possessed by a system as a whole, by observations of phenomena taking place wholly within the system.' In the explanation of various experiments to determine the velocity of the earth by terrestrial observations, sometimes this postulate and sometimes L.T.E. are invoked but, generally, the postulate alone is quoted to explain the experiments, L.T.E. being distrusted somewhat, because a few of the experiments cannot be explained by L.T.E. For example, in the experiment of Wood, Tomlinson, and Essen,³ on the change in the frequency of a rod rotated horizontally and thus subjected to different magnitudes of F.L. contraction which yielded a null result, the complete compensation of the longitudinal contraction by a change in the elasticity cannot be explained by L.T.E.

It might be said at this stage that it is precisely for such reasons that S.R. is superior to a scheme in which L.T.E. are based on a fixed aether. But wait. L.T.E., according to S.R., admittedly, should be competent to deal with cases of relative motion or one in which a change in the inertial frame occurs. Now when an apparatus is rotated or is carried by the earth from one position along its orbit to another, there is undoubtedly a change in the inertial frame. In the case of rotation of an instrument through a right angle, for example, the x and y axes exchange their positions and there are different transformation equations for the two axes. Astronomical aberration can be satisfactorily explained by S.R.⁴ only on the basis of a change in the inertial frame of the earth as it courses along its orbit. Why is it, therefore, that the *changes* in the motion of the earth in its orbit cannot be detected by terrestrial observations? L.T.E. indicate that this should be possible. After all, L.T.E. are successfully applied to all experiments in which relative motion occurs on the earth itself—for example, in the experiment of Ives and Stilwell.

According to Lorentz's ideas, we can approach the results of various experiments frankly. The first-order effects such as aberration, Fizeau's water-tube experiment and Doppler-shift are explained with the help of L.T.E. A few of the second-order, non-optical (or partly optical) experiments, such as those of Ives and Stilwell, and of Otting and increase in the life of fast mesons are likewise satisfactorily explained. The purely optical second-order experiments such as those of Michelson and Morley, and of Kennedy and Thorndike, are, of course, adequately explained by L.T.E. and, contrary to the belief of some writers, the null results of the experiments of Rayleigh

¹ Sommerfeld, *Electrodynamics*, New York, 1952, p. 227

² Whittaker, *From Euclid to Eddington*, Cambridge, 1949, p. 58

³ Wood, Tomlinson and Essen, *Proc. Roy. Soc. A*, 1937, **158**, p. 606

⁴ Leigh Page, 'On the Aberration of Light', *Astrophys. Journal.*, 1925, **61**, 70

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and of Brace on double refraction in matter passing through the aether, of Trouton and Rankine on the change in the electrical resistance of a conductor differently oriented relative to the direction of motion of the earth, and of Trouton and Noble on the mechanical forces acting on a charged electric condenser moving through the aether, can all be satisfactorily explained in terms of Lorentz's electrical theories (parts that are still valid and accepted) and L.T.E.¹ There are, however, a few experiments, such as Wood's quoted earlier, which *cannot* be explained by L.T.E.²

It is interesting to note that, although time-dilatation has been observed directly in the experiments of Ives, Stilwell, and Otting, no direct observation of F.L. contraction of length has yet been made.³ To be exact, only a change in frequency—and as measured in the 'other' system (source in S' and observer in S)—was recorded in these experiments. It is, however, assumed that the change in frequency arises from the 'slowing' of atomic processes, and that the frequency in S' would also be the same were it observed in S' because the classical Doppler effect is separated in these experiments from the time-dilatation effect. In the case of fast mesons, possibly, time-dilatation is directly observed.

To summarise, although the principle of relativity asserts that the laws of physics have the same form in all inertial systems, an examination of space and time transformations *separately* shows that the inertial system assumed as the standard of rest, in deriving L.T.E., is a preferential system. The aether qualifies for this preferential system. Although the aether has not been experimentally discovered, only its postulation makes a logical interpretation and application of L.T.E. possible.

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THE PRINCIPLE OF MINIMUM ASSUMPTION

PROFESSOR R. O. Kapp's suggested principle of minimum assumption⁴ is most alluring. Not only is it simple and apparently universal in application, it is also claimed to yield a great variety of concrete results. But no matter how much initial sympathy we may have with Kapp's thesis, the unfriendly realities must be faced. The hard facts are that the discovery of principles of such fundamental

¹ Sir James Jeans, *The Mathematical Theory of Electricity and Magnetism*, Cambridge, 1951, pp. 595-596

² Cullwick believes that M. Wilson and H. A. Wilson's experiment on a rotating magnetic insulator (*Proc. Roy. Soc. A*, 1913, **89**, pp. 99-106) discriminates between the two theories verifying only the formula based on Einstein's ideas. We shall comment on this separately. See pp. 161-174 of his book, *Electromagnetism and Relativity*, London, 1957.

³ It is not necessary for a system to be physically discovered, to sustain a physical theory. Witness various quantum theories including the postulation of neutrino. This ubiquitous particle may indeed have something to do with the aether.

⁴ R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, **8**, 265-280

character and with such far-reaching predictive and explanatory power is not as easily brought about as all that.

Here I wish to raise some of the difficulties that become apparent upon a closer inspection of Kapp's paper, and mention them in the order of increasing perplexity. It seems an inevitable conclusion that the principle, as it stands, is unacceptable. Whether or not, in some modified and more restricted form it may yet be entertained, is a matter for much careful analysis.

Kapp makes it clear at the outset that his aim is a very radical one. He intends to show that Ockham's Razor (or rather his interpretation of it) is no mere methodological rule advising scientists to adopt provisionally a procedure which may or may not lead to the correct result. The principle is a direct outcome of the objective character of the laws of nature. Thus Kapp announces:

I wish to raise it [Ockham's Razor] from a mere rule of procedure to one of the great universal principles to which the whole of the physical world conforms. At this level it would be worded as follows: *In physics the minimum assumption always constitutes the true generalisation.*¹

Further on² he defines a minimum assumption 'as one that is completely unspecific'. And this he explains by means of examples. Finally he says: '*In physics a generalisation that is logically possible is also physically possible.*'³

I propose to examine in some detail only two of the numerous concrete results that allegedly follow from the above principle. One concerns the claim that the falsity of the doctrine of 'horror vacui' follows directly from the rule of minimum assumption Kapp says:

. . . it was assumed at one time that Nature has specific likes and dislikes; for instance, that she abhors a vacuum. This could be translated into contemporary language as the specific assumption that a law of physics prevents the density of matter from falling below a specific value. But it is now known that the true generalisation about the density of matter is unspecific. The laws of physics permit any density, ranging from the high concentration that occurs in the white dwarf stars to the extreme tenuousness of extragalactic space.⁴

The second example refers to a more recent scientific result, namely to the discovery of Hafnium, the element with seventy two unit charges in its nucleus. This element remained unobserved long after the construction of the periodic table, but its existence was predicted and according to Kapp it 'was predicted purely on the basis of this principle'.⁵ For, he explains, the principle forbidding specific assumptions demands that there be nuclei with *any* number of charges, including the number seventy two. He goes even further and points out that it should have been clear that *any* includes zero as well. Consequently the existence of the Neutron, too, should have been predicted on the basis of the principle before it was actually observed.

An immediate question that arises in connection with the last example is: how do we account for the fact that there are no nuclei containing more than ninety-two

¹ R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, 8, p. 272

² *Ibid.*, p. 273

³ *Ibid.*, p. 279

⁴ *Ibid.*, p. 27

⁵ *Ibid.*, p. 276

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charges? Clearly the unspecific assumption implies that there be no upper limit to the number of charges a nucleus may have. This question is answered by Kapp himself. It is answered, however, not by insisting that the present state of affairs is merely a temporary one, and that eventually nuclei with any number of charges are going to be observed, but in the following manner:

For the elements to be found in nature the maximum number of such charges is ninety-two, a stability limit. This limit provided a logical reason why no nucleus could be observed in nature with more than ninety-two unit charges, for a greater number would be inconsistent with the definition of a stable particle.¹

These last words are somewhat puzzling. True enough, to call a nucleus stable when we can see it disintegrate in front of our very eyes 'would be inconsistent with the definition of a stable particle'. But there is nothing contrary to logic in insisting that a nucleus of ninety-eight or more charges does not disintegrate and therefore is to be found in a stable state. It is only inconsistent with observation.

It seems therefore that the only way Kapp could assign a 'logical reason' for the disintegration of such nuclei is by claiming that it follows logically from the laws of forces which apply within a nucleus, that they cannot remain stable. From this we may then infer the sense in which the expression 'logically possible' is employed in the statement 'In physics a generalisation that is logically possible is also physically possible'. The expression appears to have been used with the intention of excluding not only generalisations which are self-contradictory but also generalisations that stand in contradiction to other established propositions. In our case then, the generalisation, that the number of charges constituting the nucleus may be as great as you like, is to be regarded as 'logically impossible', as it contradicts the propositions embodying the laws of forces governing the interior of nuclei.

This last idea is of central significance. It would have to be used by Kapp in reply to many questions one might ask against his principle. For example: if the principle is valid, why is there a lower limit below which the temperature of a material system does not drop? The completely unspecific assumption is surely that the temperature of a body may be anything. How do we then account for the existence of the absolute zero of temperature? Or in the same manner we can ask: Why can a body not acquire a velocity greater than that of light, when according to the principle any velocity should be possible? Kapp does not pose these questions but one can imagine that his answer here, too, would be that propositions stating the existence of an absolute zero of temperature or the existence of an upper limit to the magnitude of velocity attainable are not independent; they follow from other given propositions. In the former case the given propositions are those constituting the kinetic theory of heat, and in the latter the special theory of relativity.

But here arises a very serious difficulty with Kapp's thesis. Is it really clear in all cases where we are confronted with two sets of propositions, which is to be regarded as 'given' and which as 'follows'? In other words, since the principle of minimum assumption only applies to 'primary' propositions which *entail* others but not to 'secondary' ones *entailed* by others, we must have some unequivocal method whereby to distinguish between the two, before the principle can be put to any use. But we were not given any such method.

¹ R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, 8, p. 276

Kapp's claim (quoted above) about the doctrine that nature abhors a vacuum is a case in point. He says that the doctrine could have been exposed as fallacious on the basis of his principle which demands that matter may exist in *any* density. But Aristotle, the protagonist of the doctrine, did not conceive it as an independent primary dogma. He presents it as a conclusion drawn from self-evident premises. For Aristotle reasons from the impossibility of infinite velocity that a vacuum is impossible. Thus, even if Aristotle were acquainted with Kapp's principle, he would not have made a generalisation permitting the existence of a vacuum, for to him that would have amounted to a logically impossible generalisation. How can we expect Aristotle to have known that his doctrine 'horror vacui' was not a secondary thesis, for it is not entailed by the true basic laws of dynamics as we know them now?

Or what is the answer to the even more difficult question, of how we really know in all the cases where we find the principle is violated, that we are dealing with secondary propositions? How do we know, for instance, that we are not to start first with the proposition that the velocity of a body may reach any degree, as demanded by the principle, take this as our primary proposition, and consequently modify all the other related proposition accordingly (even if it does mean the overthrow of the theory of relativity)? Or in the case of the periodic table of the elements, why not start by assuming, in accordance with the principle, that nuclei with any number of charges do exist and conclude that the laws of forces operating within the nucleus must be such as to allow the increase of charges to any desired value? Surely to say the laws of forces cannot be such, for we see in fact the disintegration of the heavier elements is merely begging the question.

Perhaps the above difficulty is not necessarily insurmountable. However, it certainly cannot be overlooked either. It cannot be taken for granted without adequate analysis that there is an objective and not merely an arbitrarily adjustable difference between primary propositions (to which the principle is to be applied) and secondary propositions (which are logically pre-determined and may therefore contradict the principle).

2

A difficulty of another kind is that of identifying a minimum assumption. When presented with alternative hypotheses, is it always quite clear which one contains the minimum assumption? True enough, Kapp is very reassuring on this point. He says:

. . . one may learn the operative word by which to recognise a minimum assumption. It is 'any'. It need not apply only to numbers but can also apply to quantities, properties, relationships, configurations, to any feature that one likes to mention. When there are only two alternative possibilities, the grammatical substitute for 'any' is 'either' as, for instance, when there is a choice between the positive and negative sign. So a minimum assumption can be recognised by the use in its formulation of the words 'any' or 'either'.

In practice it is not difficult to distinguish between a minimum assumption as just defined, and one that is not a minimum one.¹

¹ R. O. Kapp, 'Ockham's Razor and the Unification of Physical Science', this *Journal*, 1959, 8, p. 274

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But surely matters are not as simple as this. The same idea can often be formulated in different ways. The wording alone cannot be a sufficient guide in the selection of the correct hypothesis. For if it were only the word 'any' that mattered, I cannot see what prevents us from asserting 'matter can be found *anywhere*'—thus entailing the impossibility of a vacuum, or even 'any gap may exist in the periodic table of elements'—thus destroying the basis for the prediction of Hafnium.

3

I believe, however, that the main trouble with the principle is that Kapp has not laid down clearly the constraining conditions under which it is to operate. Just as in mathematics a minimum principle is useless without constraining conditions, so Kapp's principle, as it stands, is indeterminate. And this can best be demonstrated by showing how it admits all sorts of absurd questions.

Why, for example, does the periodic table not contain elements with fractional charges in the nucleus? We are told that by the use of the word 'any' we were made aware of the existence of Hafnium (72 charges) and the Neutron (0 charges), why not also nuclei with half or quarter of a unit charge? The answer that electric charges exist in discrete amounts only raises the more embarrassing question: why indeed? According to Kapp's principle we are to expect to find electric charges in *any* amount. Radiation too, of course, should be emitted in *any* quanta.

Similarly, why are there not *any* kinds of masses—masses which attract or repel each other according to *any* laws of distance? And if it be answered that even Newton's Law of Gravitation is not a primary law, then what is? The Law of Conservation of Energy? No, because it contradicts Kapp's Principle. For the minimum assumption is surely that all kinds of closed isolated systems should exist. There should be closed isolated systems in which the energy either increases or decreases and not only such systems in which the energy always remains the same.

It is evident, therefore, that the first and most essential step towards clarifying Kapp's principle is to give a clear statement of the constraining conditions under which it is to apply. In its present form the principle of minimum assumption is a principle of no assumption—leading only to an assertion of the type 'anything may happen anywhere, at any time'. Or more simply 'in physics anything goes'.

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THERE are, perhaps, some questions in physical science that cannot be so worded as to provide only two possible answers; yes or no. But most questions can be so worded provided sufficient trouble be taken with their formulation. They then sometimes appear with uncompromising clarity and prove to be such that each of the two possible answers is unattractive. Whichever answer has the support of facts and logic, it will

then be deprecated by those who forget that the alternative answer would earn their disapproval too. A natural reaction to such disconcerting questions is to say that the subject is not as simple as all that, or that the answer found could only be entertained in some modified or restricted form, or that the question is not a real one, or to try to find an answer yes that is not quite yes, an answer no that is not quite no. Such efforts are sterile. They only blunt the sharp edge of the question. They offer a comforting confusion as an escape from the stern demand for a choice between unwelcome alternatives.

These remarks are relevant to Mr Schlesinger's note. I shall begin my comment on it by rewording a distinction that is to be found implicitly in the paper by myself to which he refers. The rewording may help to reveal that the choice is inescapable.

Let all general statements in physics be placed in one of two classes, Class A and Class B. Such statements may be of any kind; they may be about basic principles, about laws, about quantities, about cosmic constants; it is only required that they have universal validity.

Those to be placed in Class A are defined as the statements that can be inferred from other statements. This may be because they are tautologies, inferable from the words used for their formulation. It may also be because they are implicit in more general statements, and inferable from these. Thus the laws of planetary motion belong to Class A because they are implicit in the more general laws of mechanics. Statements in this Class are called 'reducible'. They are said to be 'explained' by the more general statements in which they are implicit. Hence they could become known without the aid of observation and by pure inferential reasoning provided this were comprehensive and acute enough and the general statements in which they are implicit are known.

Statements in Class B are not tautologies and are not implicit in any others. They are called 'irreducible'. They cannot be 'explained' in the sense in which a statement in physics is said to be explained. They cannot be arrived at by inference or tested by deductive processes. They are only known by observation and their validity can only be confirmed by observation. Thus the laws of planetary motion appeared in Kepler's day to belong to Class B. Kepler found them by observation and his contemporaries were in a position to confirm them by observation. Much later Newton showed that these laws could be known by inference to a person who spent his life under a blanket of cloud and had no means of observing planets. He could deduce the elliptical orbit of planets from more general principles. Therewith the laws of planetary motion were transferred from Class B to Class A. I have described Class B statements metaphorically as clauses in a Cosmic Statute Book. It is clauses of this statute book kind, and of this kind only, that ensure order in human affairs.

In the history of physical science more and more statements, once thought to belong to Class B, have been found really to belong to Class A. This has been expressed by saying that more and more observed facts have been 'explained'. The explanation has always been in terms of more general statements, like Newton's explanation of the elliptical orbits of planets in terms of the more general laws of mechanics. One cannot yet show that *all* statements in physics belong to Class A. Some are in doubt. Among them are certain very general principles and a handful of cosmic constants: the velocity of light, the quantum of action, unit electric charge,

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and a few others. They are known by observation only. It has not yet been found possible to prove that anyone who did not make, or know of, the necessary measurements could nevertheless infer the precise value of these constants from general principles, as he could infer the elliptical paths of planets from more general principles.

The uncompromising, the disconcerting, the unwelcome question is this: Can the process of transferring statements from Class B to Class A continue until none are left in Class B? Do all the statements that have any place in physics really belong to Class A, even when they seem at present to belong to Class B? Are all statements in physics reducible? Can every one of them be explained in the same way as the elliptical orbits of planets have been explained? Is there, in physics, nothing to which the metaphor of a Cosmic Statute Book applies? Are the laws of physics of a different kind from those laws that demand certain things and prohibit others? Is there no principle in physics to make for specified order in the sense in which the laws of a country make for specified order?

The above questions are but different forms of one single question. What is the answer: yes or no? As I showed in my paper the answer 'yes' leads to the Principle of Minimum Assumption; the answer 'no' is a denial of this principle.

If one thinks only of some of the implications of this question one is certainly inclined to answer 'yes'. If one thinks only of other implications one is inclined to answer 'no'.

If the correct answer is 'yes' one may hope some day to explain why such cosmic constants as the velocity of light, unit charge, the quantum of action have precise values; why there is a principle of conservation energy; to answer sundry questions beginning with 'why' to which no answer is yet forthcoming. If the correct answer is 'no' some at least of these facts are intrinsically irreducible; they will never be explained. The reason why they are observed can only be that an arbitrary requirement, a clause in the metaphorical Cosmic Statute Book, leaves no alternative. When one remembers this one likes the answer 'yes'. It is a hopeful answer.

If yes is the correct answer and if there is no law or other fact of physics to which the metaphor of a statute book applies, there can be nothing in the physicist's universe of discourse by virtue of which order is imposed on the material universe. Provided only that the principles of logic be met, physics must then be a science in which, to use Mr Schlesinger's term, 'anything goes'. If it is so the physicist who hopes, by observation and experiment, to discover the laws of nature, to transcribe the requirements of the metaphorical Cosmic Statute Book into the textbooks on physics, is not pursuing the only course towards understanding of his subject. Pure inference would provide an alternative course. While the physicist is believing it to be his duty to find out what is prohibited by the laws of physics he could, instead, seek to find out what is prohibited by the principles of logic; both quests would lead him to the same conclusion. Many do not like to think that it is so. When one remembers this one is likely to follow Mr Schlesinger and hope that the correct answer is 'no', to deprecate with him the notion that, in physics, 'anything goes'.

There is the dilemma. Human nature being what it is there is a temptation to dodge so irksome a question. But I am trying very hard so to present the problems that the question can no longer be dodged.

It is against this background that Mr Schlesinger's note has to be considered. I analyse his various points below:

- (1) 'The answer found by myself could only be entertained in some modified or restricted form.' This is to express the hope that one may be able to find an answer 'yes' that is not exactly yes.
- (2) 'The distinction between what is given and what follows, between a primary and a secondary proposition, is not clear.' It is admittedly difficult to find a good criterion for this distinction and so to word it that it is absolutely mistake proof. But such difficulty is no argument against seeking a criterion. Without a criterion that can withstand rigorous philosophical criticism one is still able, in practice, to distinguish quite easily between a broad generalisation and a more restricted one. Would anyone deny that the laws of mechanics are more general than the law requiring planets to have elliptical orbits? It can be shown that the law governing the maximum thermal efficiency obtainable from a given steam engine follows from the principle of least action. No-one could argue rationally that this very broad principle follows from the performance of steam engines.
- (3) 'By the Principle of Minimum Assumption the periodic table could contain elements with fractional charges on the nucleus.' Admittedly the fact that the elements do not contain fractional charges could be a refutation of the Principle of Minimum Assumption. That the 'charges' are always whole numbers would then be a Class B statement. It is also true that, at the present state of knowledge, the quantised nature of electric charge has not yet been 'explained'; it has not been inferred from anything more general; it appears as an irreducible fact; it is one of the few remaining statements in physics that might have to be called a clause in the metaphorical Cosmic Statute Book. But I doubt whether many physicists would agree with Mr Schlesinger that it must always be so. The search for a unifying principle that will 'explain' the precise value of unit charge has not yet been abandoned.
- (4) 'An inference from the Principle of Minimum Assumption would be that matter can be found anywhere and this would entail the impossibility of a vacuum.' No. It would only be so if one word were as good as another; if 'can' were a synonym for 'must'; if 'anywhere' were a synonym for 'everywhere'. The statement that matter *must* be found *everywhere* would entail the impossibility of a vacuum, but it would not conform to the Principle of Minimum Assumption.
- (5) 'An inference from the Principle of Minimum Assumption would be that any gap may exist in the periodic table of elements.' This is a more subtle lapse from logic. The error is to assign to this statement the same status as to the statement that any atomic nucleus may carry any number of unit charges up to the stability limit. The first statement is about a man-made device for recording and arranging facts, namely the periodic table. It is not a statement about physical realities. The second statement is about physical realities, namely atomic nuclei. The Principle of Minimum Assumption applies only to this latter kind of statement.

REGINALD O. KAPP

SOME COMMENTS ON 'IMAGINARY EXPERIMENTS'

IN appendix* xi of *The Logic of Scientific Discovery* Professor Popper undertakes a critical analysis of imaginary experiments, mostly pertaining to quantum theory. With regard to Heisenberg's γ -ray microscope experiment which has become the

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classical illustration of the principle of indeterminacy, Popper mentions several points which in his opinion Heisenberg tried to establish, among them 'the *disturbance* of the measured object by the process of measurement, *whether of position or of momentum*' (p. 451). Popper arrives at the conclusion that Heisenberg's experiment is inadequate as a basis for interpreting the indeterminacy relations, for 'it fails to establish that measurements of position and momentum are symmetrical' as they should be when conforming to the symmetrical structure of the relation $\Delta x \cdot \Delta p \sim h$. The asymmetry referred to consists, according to Popper, in the following: In determining the electron's position one has to use light of short wavelength which leads to a *disturbance* of the electron's *momentum*. If, on the other hand, one wants to determine the momentum, one must use long wavelength radiation, and the resulting observation will *fail to disclose* the electron's position which will thus remain indeterminate not as the result of a disturbance.

Popper's argument is invalid because it is based on the erroneous assumption that it is the *disturbance* of either the position or the momentum which is the essential feature of the mechanism of interaction between the object of observation and the agency of measurement. A thorough analysis of the problem in question, amplifying on Heisenberg's explanation, has been given more than thirty years ago by Niels Bohr.¹ The main points may be presented as follows: One has to use different experimental arrangements depending on whether one wants to measure *either* the electron's position *or* its momentum—a microscope in the first case and a spectroscope in the second, and in both of them one has to take due regard of the Compton effect as well as of the limitations inherent in the means of observation.

In the case of the microscope (determination of position) the uncertainties of position and momentum involved are respectively $\Delta x = \lambda / \sin \alpha$ and $\Delta p = h \sin \alpha / \lambda$, where $\sin \alpha$ is the numerical aperture of the microscope. It is thus the finite aperture of the microscope (and not the 'disturbing' wavelength of the illuminating light alone) which is responsible in a reciprocal manner for both uncertainties. On the other hand, in determining the electron's momentum with the spectroscope, one has to measure the wavelength of the scattered radiation, and if this is done in the direction opposite to the incident light, the uncertainties of position and momentum involved are respectively $\Delta x = 2h / \lambda mc$ and $\Delta p = \lambda mc / 2l$, where l is the length of the wave train used in the observation. The shorter the latter compared with the wavelength used, the greater the inaccuracy $\Delta \lambda$ of the determination of the wavelength, and therefore of the momentum (in fact, $\Delta \lambda = \lambda^2 / l$). It is thus the finite extent of the wave train scattered by the moving electron and analysed by the spectroscope, which is responsible in a reciprocal manner for the uncertainties both of momentum and position. A complete symmetry of the uncertainties is thus prevailing in conformity with the formula, if one takes into account *all the details* of the respective experimental arrangements.

It is interesting to note that, in the case of the latter experiment, the accuracy of the determination of the momentum is the higher, the *shorter* the wavelength λ . This, however, implies a *greater* (controlled) disturbance of the momentum h/λ of the observed particle. Contrary to popular belief, therefore, it is not the magnitude of the disturbance itself which determines the inaccuracy of the measurement.

¹ *Nature*, 1928, **121**, 580; cf. especially 582-583

In another passage of the same appendix, Professor Popper warns against what he terms the 'apologetic use of imaginary experiments'. The warning is well worth to be taken to heart; it is only regrettable that the example chosen (p. 447) is not a suitable illustration. Popper quotes Einstein's imaginary experiment, devised to demonstrate the possibility of measuring simultaneously time and energy of a system with as great an accuracy as desired. The gist of the experiment is that a box from which a photon is released at a given time, is being weighed before and after the release, whereby use is made of the formula $E = mc^2$. Bohr refutes Einstein by using arguments from general relativity (change of the rate of a clock when displaced in the direction of the gravitational field). Popper objects to Bohr's argument, because of the 'extravagant assumptions' made by him in introducing general relativity in order to discard an experiment based on special relativity. But this objection is unfounded, for although the relation $E = mc^2$ is derived from special relativity, it is Einstein himself who introduces gravitation into the experiment by weighing the box and thus making use of the identification of inertial and gravitational mass in this formula. Bohr therefore quite legitimately amplifies on this argument by a further analysis of its consequences, based on general relativity.

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SCIENCE AND PERSONS

I

PROFESSOR Polanyi's Gifford Lectures¹ for 1951-52 form an extremely interesting and learned book which covers a great deal of ground—science, politics, law, art, philosophy, and religion—all are involved to various extents. But it is an extremely difficult book to review for a number of reasons. First, because it attempts to face up to difficulties which works on the philosophy of science often seek to avoid. These difficulties are all connected with the fact that *persons* are involved in the making of science and also enter into the subject-matter of some of its branches. The process of personification—applied even to inanimate objects—which was so prevalent in the ancient world, obviously had to be abandoned if physical science was to develop. A tradition was thus established which has persisted even up to the time of the rise of a science of persons, so that it is perfectly legitimate to be ornithomorphic in studying persons if you find observations on seagulls helpful; but it is quite heretical to be anthropomorphic, even when you are studying *ἄνθρωπος* himself. The upshot of all this is that if we want to know what other people think about persons it is not to science but to literature that we must go. It is also customary to *abstract* from the fact of *participation* of persons in science, even when we are discussing the philosophy of science. It is becoming recognised, nevertheless, that a three-termed relation is involved in the pursuit of science, the three domains of which are: (i) persons; (ii) language used by persons; (iii) the subject-matter to which the language refers. If we abstract from the first and third we are said to be talking syntax—we are dealing only with the language, without reference to the persons using it or to the subject-matter referred to. If we ignore persons and deal with the language in its relation to subject-matter then we are pursuing semantics; and if we take into account the rôle of persons then we are engaged in pragmatics. So far pragmatics seems to be the least studied of these topics, and also to be dominated largely by the behaviouristic attitude which itself in effect abstracts from persons as such. It will be seen, therefore, that Polanyi, in not availing himself of the customary methods of avoiding the notion of person, has set himself a very difficult task. A second feature of this book which creates difficulties for the reviewer is the method of presentation, which is intuitive rather than

¹*Personal Knowledge: Towards a post-critical philosophy.* By Michael Polanyi. London: Routledge and Kegan Paul, 1958. Pp. xiv. + 428. Price 42s. net.

analytical. Sometimes highly debatable assertions are made with little or no preparation, such as: 'tautologies are necessarily true, but mathematics is not' (p. 187) or 'we are jointly aware of the retinal image and of the adaptive effort' [of accommodation] (p. 362); or 'we shall see that biology is an expansion of the theory of knowledge into a theory of all kinds of biotic achievements' (p. 347). Such an assertion may then be succeeded by another statement which is said to follow from the former one, although the connection between the two statements is not always clear (at least to one dull-witted reviewer) and the following of one from the other far from obvious. Thirdly, the author uses a somewhat unusual vocabulary. For example, abundant use is made of the following words: articulation (not in the anatomical sense); achievement (not in the heraldic sense); coefficient (not in the mathematical sense); convivial (not in the dictionary sense); comprehension, commitment, fiduciary, focal observation, subsidiary awareness, passion and passionate, rational (in several senses). These are all key words, essential to the understanding of the book; but the reader is not in all cases told in what senses they are used. It is hoped therefore that the following notes may be helpful. There is a good index and a dictionary is useful but not in every case. For example, according to the *Shorter Oxford English Dictionary* the word *convivial* means 'of or belonging to a feast or banquet, festive; fond of feasting and good company, jovial,' (as we should expect from the meaning of the Latin word *convivium*). But Polanyi does not use it in this sense at all. In the section entitled 'Pure Conviviality' of the chapter headed 'Conviviality' we are told nothing about feasts or banquets, but on page 209 we read of 'the primitive sentiments of fellowship that exist previous to articulation among all groups of men and even among animals. Evidence of the primordial character of such conviviality and of the lively emotions engendered and gratified by its interplay is supplied by the experience both of animals and men.' Also on the same page we learn that 'a newly hatched chicken soon learns to join the flock around its mother and to seek protection under her wings'. On the next page we are reminded that 'The comprehensive scope of convivial relations has been expressed by Köhler in the aphorism that a solitary chimpanzee is not a chimpanzee', and that 'Companionship among men is often sustained and enjoyed in silence'. The author thus appears to have generalised the dictionary meaning of 'convivial' by not restricting it to feasting. He is using it in contrast to 'objective' in one sense of this word. Our attitude towards our fellow creatures is objective in this sense if it is that of a detached observer, outside the system (which includes those fellow creatures) under observation but having no part in it; and it is convivial if it is one of fellowship with them, accompanied by a feeling of 'being in it together'.

The word commitment is used in the dictionary sense of 'engagement

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that restricts freedom of action'. Thus the author speaks of his 'commitment to personal knowledge' (p. 327) distinguishing between 'the personal in us, which actively enters into our commitments, and our subjective states, in which we merely endure our feelings. This distinction establishes the conception of the *personal*, which is neither subjective nor objective. In so far as the personal submits to requirements acknowledged by itself as independent of itself, it is not subjective; but in so far as it is an action guided by individual passions, it is not objective either. It transcends the distinction between subjective and objective' (p. 300). On page 363 we see how widely this notion also is generalised: '. . . biology has been revealed as an appreciation of commitment. To swallow something in the hope that it may be wholesome is clearly a commitment, and so is every act of seeing things in one particular way. I have suggested . . . that . . . commitment may be acknowledged even at the vegetative level, since it is of the essence of a living organism that each part relies for its function on the presence and proper functioning of a number of other parts . . . Commitment may then be graded by steps of increasing consciousness; namely, from *primordial*, vegetative commitment of a centre of being, function and growth, to *primitive* commitment of the active-perceptive centre, and hence . . . to *responsible* commitments of the consciously deliberating person. . . . Biology is a responsible commitment which appraises other commitments.'

The chapter headed 'The Logic of Achievement' covers such diverse topics as the distinction between right and wrong working of a machine or the organs of an animal's body; between causes and reasons; between logic and psychology; originality in animals; equipotentiality in embryos; and (strangely enough) logical levels in the sense of the distinction between science and metascience. The use of the word achievement is illustrated by the following passage on page 357:

' . . . I have distinguished two kinds of biological achievements, namely, (1) achievements performed by the rational concurrence of several parts with fixed functions and (2) achievements performed by the equipotential interplay of all parts of a system. . . . Embryogenesis appears to be a comprehensive achievement due to the rational combination of these two types of rational principles.

The analysis of the process by which living beings are formed corresponds to the logic of achievement, as illustrated by the manner in which we find out how a machine works. . . .

The meaning of experimental embryology is thus dependent on personal knowledge: both in respect of the unspecifiable knowledge of true shapes, and in respect of the appreciation of the process by which highly significant shapes and structures are brought into existence. This situation has caused uneasiness among scientists.

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The word *articulate* is used in the sense in which it is used by Edward Fitzgerald in

And, strange to tell, among the Earthen Lot
Some could articulate, while others not.

The chapter headed 'Articulation' begins with the types of learning in animals, passes on to the use and abuse of language in human relations and ends with a section on mathematical heuristics. There is an error on page 86 in this chapter. Instead of saying: 'the infinite set $\aleph$ counts all numbers' it would be more correct to say: ' $\aleph_0$ is the cardinal number of the set of all natural numbers' ('all numbers' would include real and complex numbers); and 'next larger infinite set' is meaningless; what is meant is 'next larger cardinal number'. Also $\aleph_2$ enumerates the set of all real functions of a real variable, but the set of all curves has cardinal $\aleph_1$.

The ways in which some of the other words mentioned above are used may be illustrated by the following, from the Preface:

I regard knowing as an active comprehension of the things known, an action that requires skill. Skilful knowing and doing is performed by subordinating a set of particulars, as clues or tools, to the shaping of a skilful achievement, whether practical or theoretical. We may then be said to become 'subsidiarily aware' of these particulars within our 'focal awareness' of the coherent entity that we achieve. Clues and tools are things used as such and not observed in themselves. They are made to function as extensions of our bodily equipment and this involves a certain change in our own being. Acts of comprehension are to this extent irreversible and also non-critical. For we cannot possess any fixed framework with which the re-shaping of our hitherto fixed framework could be critically tested.

Personal knowledge is an intellectual commitment, and as such inherently hazardous. Only affirmations that could be false can be said to convey objective knowledge of this kind.

I have shown that into every act of knowing there enters a passionate contribution of the person knowing what is being known, and that this coefficient is no mere imperfection but a vital component of his knowledge.

The word 'fiduciary' seems to be used in the sense given in the *Shorter Oxford English Dictionary*, 'of the nature of, proceeding from, or founded on trust or confidence'. As examples of its use we may quote:

The fiduciary passions which induce a confident utterance about the facts are personal, because they submit to the facts as universally valid, but when we reflect on this act non-committally its passion is reduced to subjectivity (p. 303).

A fiduciary philosophy does not eliminate doubt, but (like Christianity) says that we should hold on to what we truly believe, even when realizing the absurdly remote chances of this enterprise, trusting the unfathomable intimations that call upon us to do so (p. 318).

Throughout the book the author seems to be haunted by the rival claims of belief and doubt and by the inescapability of both. On page 109

he says: 'I am attempting to resolve by conceptual reform the apparent self-contradiction entailed in believing what I might conceivably doubt.' The author is opposed to positivistic and utilitarian attitudes towards science while criticising also what he calls *Objectivism*. What he means by this may perhaps be gathered from the following passage on page 16:

. . . modern man has set up as the ideal of knowledge the conception of natural science as a set of statements which is 'objective' in the sense that its substance is entirely determined by observation, even while its presentation may be shaped by convention. This conception, stemming from a craving rooted in the very depths of our culture, would be shattered if the intuition of rationality in nature had to be acknowledged as a justifiable and indeed essential part of scientific theory. That is why scientific theory is represented as a mere economical description of facts; or as embodying a conventional policy for drawing empirical inferences; or as a working hypothesis, suited to man's practical convenience—interpretations that all deliberately overlook the rational core of science.

On the next page the author speaks of the 'indispensable intellectual powers and their passionate participation in the act of knowing.' It is to these powers and to this participation that I am referring in the title of this book as "Personal Knowledge". He has already given hints of his own attitude.

To say that the discovery of objective truth in science consists in the apprehension of a rationality which commands our respect and arouses our contemplative admiration; that such discovery, while using the experience of our senses as clues, transcends this experience by embracing the vision of a reality beyond the impressions of our senses, a vision which speaks for itself in guiding us to an ever deeper understanding of reality—such an account of scientific procedure would be generally shrugged aside as out-dated Platonism: a piece of mystery-mongering unworthy of an enlightened age (p. 5).

. . . by this time it should be clear how far-reaching are in my own opinion the changes in outlook that are required in order to establish a stable alternative to the objectivist position. I cannot hope to do more in this book than to exhibit a possibility which like-minded people may wish to explore.

I shall go on, therefore, to repeat my fundamental belief that, in spite of the hazards involved, I am called upon to search for the truth and state my findings (p. 315).

But is not the search for truth in natural science a search in which we never *know* when we have caught up with our quarry? We do not *know* which of our current beliefs are true because we do not *know* what discoveries the future may have in store for us which will require their revision. Is not this the reason why we must be content with belief? On page 313 Polanyi says:

Only an activity can go wrong, and all activity incurs the risk of failure. To believe something is a mental act: you can neither believe nor disbelieve a passive experience. It follows that you can only believe something that might be false. This is my argument in a nutshell; I shall now elaborate it in some detail.

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But in the course of this elaboration we are presented, on page 315, with the following statements which seem (to the present reviewer) to throw the matter into complete confusion:

Therefore, though every person may believe something different to be true, there is only one truth. This can be substantiated as follows. The function of the word 'true' is to complete such utterances as "' p ' is true", which are equivalent to an act of assent of the form 'I believe p '. [On the next page we are told that] 'The concept of commitment postulates that there is no difference, except in emphasis, between saying 'I believe p ' or "' p ' is true'. Both utterances emphatically put into words that I am confidently asserting p as a fact. This is something I am doing in the act of uttering the words in question, and is quite different from my reporting that I have done this in the past, or that somebody else has either done this or is doing it now. If I report "I believed p " or " X believes p ", I am not committing myself in respect to p and hence no utterance linking " p " to "true" corresponds to these reports; they issue in no assertion of the sentence p as true, be it in relation to my own past or to other people's beliefs. There remains therefore only one truth to speak about.

Polanyi has much to say about the Laplacean intelligence which 'would embrace in the same formula the movements of the largest bodies in the universe and those of the lightest atom'. His remarks about 'the menace to all cultural values, including those of science, by an acceptance of a conception of man derived from a Laplacean ideal of knowledge and by the conduct of human affairs in the light of such a conception' recall what I. D. Suttie wrote about the taboo on tenderness in *The Origins of Love and Hate* and what Aldous Huxley says about organised lovelessness in *The Perennial Philosophy*.

The last of the four parts into which Polanyi's book is divided is chiefly devoted to problems of biology and biological methodology. The notion of person was eliminated from physical thinking at an early stage; how far it, in one form or another, should be retained in biology has been a matter of heated debate for centuries. For a long time orthodoxy in biology has required close adherence to the example of physics both in method and in explanatory hypotheses. It has been much easier to follow a successful model and to borrow ready made hypotheses than to invent new ones suggested by the observed subject-matter. No alternative to this procedure in biology has so far had a sufficiently spectacular success to persuade biologists to follow it. On all these topics Polanyi makes interesting suggestions. His book is a challenge to orthodoxy in many fields. We may conclude with his own statement of the aim of his book which is given on page 381:

Its aim is to re-equip men with the faculties which centuries of critical thought have taught them to distrust. The reader has been invited to use these faculties and contemplate thus a picture of things restored to their fairly obvious nature. This is all the book was meant to do. For once men have been made to realise

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the crippling mutilations imposed by an objectivist framework . . . many fresh minds will turn to the task of reinterpreting the world as it is, and as it then once more will be seen to be.

But will our politicians with their dangling deterrents and misguided missiles—themselves among the products of the objectivist framework—give us time in which to use these faculties?

J. H. WOODGER

SIR RUSSELL BRAIN ON MODES OF APPREHENSION

THE subject of Sir Russell Brain's Riddell Memorial Lecture¹ is 'Modes of Apprehension', and the modes discussed are sense-perception, both normal and hallucinatory (chaps. I and II) aesthetic experience, and finally but rather briefly religious experience (chap. III). All these are contrasted with scientific inference, which he sometimes calls 'comprehension' and sometimes 'indirect apprehension'.

The first two chapters (I: 'Vision and Fantasy' and II 'The Nature of Perception') may be considered together. The author's main aim here is to insist that the sense-datum theory is the only one which is compatible with the well-established neurological facts, and also the only one which is compatible with the phenomenological facts about hallucination. His own version of the sense-datum theory, here as in his other writings, is a representationalist theory roughly similar to Locke's.

Quite often, however, he prefers to speak of perceptual objects rather than sense-data. But it is never made quite clear what the relation between the two is. I take it that a perceptual object is some sort of complex or family of sense data, and not just a single sense-datum. It is also private to a given percipient (this is explicitly stated in a number of passages of the book). But now suppose I have two successive views of the same house as I walk around it. Is my perceptual object the same on both occasions? The sense-data are certainly different. If the perceptual object is the same, it would be public, as it were, to different points of view and different occasions of sensing, even though private to a single percipient. Moreover, if there is a temporal interval between the two successive views, during which I have no sense-data of the appropriate sorts (because I am asleep, or looking in another direction) do we still say that my perceptual object is the same on

¹ *The Nature of Experience*, Riddell Memorial Lecture, Thirtieth Series, delivered at King's College in the University of Durham, 1958, by Sir Russell Brain, Bt., Oxford University Press, 1959, pp. 73, 8s. 6d.

both occasions? If we do, it will follow that possible sense-data, as well as actual ones, must be mentioned in our definition of the term 'perceptual object'. We shall have to borrow some of the analytical techniques of the phenomenalist—and encounter some of his difficulties. Again, the same *physical* object can stimulate different sense-organs. I see and touch a pencil and tap with it on the table. Are these visual, tactual, and auditory sense-data all included within the same perceptual object?

Such questions, no doubt, are more easily asked than answered; and similar questions arise if we speak of 'the perceptual world' (p. 24 and elsewhere) and contrast it with the physical world. They also arise if we distinguish, as Sir Russell does, between private perceptual space and physical space (p. 36 and elsewhere). I take him to mean that each percipient has just one private perceptual space in which all his sense-data which have spatial properties at all are located, no matter what sense-modality they belong to, though my perceptual space would still be different from yours, and both would be different from the space of the physical world. In that case, a perceptual space would be private but inter-sensory: public, as it were, to several different senses of the same percipient, though not public to different percipients. But it might be argued, and frequently has been, that even a single percipient has several different perceptual spaces—a visual one, a tactual one, a kinaesthetic one. Or should we say that each percipient has several different *sensible* spaces but only one single perceptual space, though all of them are alike in being private to him?

These questions again are more easily asked than answered, though Sir Russell is no doubt right in holding that *some* distinction between perceptual space and physical space is essential to the sense-datum theory.

It should be added that his account of private perceptual space has one rather peculiar feature. He certainly holds that perceptual space and physical space are different spaces. On the other hand, he also thinks that we can properly ask the question 'Where is the perceptual space of each of us?' (pp. 40-41)—and answer it too. The answer is that Mr A's private perceptual space is in his brain: not, of course, in his perceptual brain (if we can properly speak of such a thing) but in his physical brain, inside his physical head. But in that case his perceptual space is just a part of physical space, the part of it which happens to be occupied by his physical brain or by some portion of his physical brain. And then there are not after all two different spaces, or two different sorts of space, perceptual and physical. There is only one—physical space—and the perceptual spaces of the various percipients are just a number of small regions within it. Moreover, when Mr A. is said to have a *private* perceptual space, it now turns out that what is private is not the space itself, but only its contents, the sense-data or perceptual objects which he alone is aware of; and not all its contents either, for some of them are physical objects, e.g. brain cells. The same conclusion is

suggested by the analogy of the ship's radar screen (pp. 41-42): 'Here *in a minute space in the vast bulk of the ship* is its (the ship's) private perceptual space' (my italics).

It seems to me that if we wish to maintain that perceptual space and physical space are 'different' spaces (and similarly that my perceptual space is a different space from yours) we must give a stronger sense than this to the word 'different'; namely, a sense which entails that though perceptual objects have spatial relations to one another, and physical objects also have spatial relations to one another, there are no *spatial* relations between any perceptual object and any physical object, whatever other relations there might be between them, e.g. similarity, concomitant variation, causal dependence. In that case the question, 'Where is the perceptual space of each of us?' would be devoid of meaning. At the best it would be a misleading way of asking a causal question, not a spatial one. Similarly, if my perceptual space is a different one from yours, my perceptual objects have spatial relations to one another and your perceptual objects have spatial relations to one another; but there are no spatial relations between my perceptual objects and yours. According to Sir Russell, however, there are. If my physical head is twelve physical inches away from yours, and we are standing back to back, my present visual field is a little more than twelve physical inches away from yours, allowing for the thickness of our respective skulls.

Sir Russell is of course well aware that the representationalist version of the sense-datum theory has been severely criticised by philosophers from Berkeley's time to the present day, and in chapter II he replies to these criticisms. The most damaging one of all is also the most familiar: 'If perception provides us only with images or representations of physical objects, how can we know that physical objects exist to cause such images or representations?' (p. 42, quoting Mr R. J. Hirst). Sir Russell claims that there is a complete answer to it, and his exposition of this answer (pp. 31-42) is perhaps the most interesting part of the book. First, he says, we must be careful to choose the right starting-point for our theory of perception. 'The fact with which we must begin is the fact of knowledge, experience or information. . . . If we start with knowledge or experience, we start with the subject-object relationship already given. We do not need to ask how we become aware of things outside ourselves because it is with that awareness that we begin' (pp. 31 *ad. fin.*-32).

So far, so good. Most contemporary philosophers would agree that this is the right starting-point for an epistemological enquiry. But in the very next sentence we leave awareness behind, and our attention is turned to quite a different topic—namely the degree of likeness which there is between sense-data and the physical objects which are their remote causes. In chapter I it was the *unlikeness* of the two which was emphasised. The

conclusion reached there, on neurological grounds, was that 'the qualitative features of a percept can have no resemblance to the physical object which it represents' (p. 24). But—I think this is the point—we must remember that their qualitative features are not the only ones they have. They have what might be called structural features as well. So now, in chapter II, we are warned against 'the error of supposing that the sense-data generated by the brain must be *entirely* unlike the physical objects which, by acting upon the brain from the outside world, produce them' (p. 32). On the contrary, if we consider how the human brain works, and its evolutionary history, we find that one of its most important functions is 'to provide us with an accurate representation of the spatial structure of the external world as well as of our own bodies'—including 'the ever-changing relationship between our bodies and their environment' (p. 32).

Here we seem to be back in the thought-world of another very distinguished medical philosopher, John Locke, asking much the same questions as Locke asked in his celebrated discussion of Primary and Secondary Qualities (*Essay*, Book II, chapter VIII), and giving much the same answers: with only this difference, that Locke's primary qualities are regarded as structural characteristics of sense-data rather than qualities. Surely it is clear that a mere *de facto* relation of resemblances between certain features of our sense-data and the spatial structure of the external world does not give us all we need, no matter how exact the resemblance is? 'But', it may be said, 'we agreed to start with the fact that we are aware of things outside ourselves.' So we did. But we seem to have ended with something which makes that fact inexplicable.

Let me try to put this point in a more constructive way. If being aware of things outside ourselves consists in experiencing sense-data which represent them, we shall have to use some phrase like 'giving knowledge of' in our definition of the crucial term 'representing'. 'X represents Y' cannot *just* mean 'X is in certain respects like, or very like, Y'. This may be part of what it means, but it cannot be the whole. In addition, we shall have to say that experiencing the resembling entity X *ipso facto* gives us knowledge of the entity Y which X resembles, or at least of Y's spatial structure. To put it in another way, the awareness of things outside ourselves must be, so to speak, built into the concept of 'representing' - *ab initio*, if a representational theory of perception is to work. Perhaps this, or something like this, is what Sir Russell himself intends to say. But he has not said it with either the clarity or the emphasis which are needed, if his version of the theory is to escape the difficulties which Locke failed to solve. There is, however, an interesting passage later in the book which is helpful: 'Assuming that in general the number, discreteness, and movement of objects we perceive are objective, science infers a structure of the physical world such that some perceptual elements are shown to be subjective. The

self-contradictory view that begins with objects and ends by making them entirely subjective is a misrepresentation of the sense-datum theory' (pp. 42 *fin.*, 43). Here the first clause of the first sentence 'Assuming that in general . . . ' throws light on what was meant earlier by saying that 'the awareness of things outside ourselves' is something with which we begin.

The replies to some other philosophical criticisms are not altogether successful. For example, Sir Russell does not appear to have understood Professor Ryle's reasons for saying that 'seeing a tree is not an effect' (p. 26) and later accuses him of taking an 'introspectionist view of perception' (p. 31)—a strange charge to bring against Professor Ryle, of all people. Professor Ryle's point of course is that seeing is not a process, and therefore not an effect, because 'see' is a success-word and not a process-word. The correct answer to this is, I think, that the word 'see' has other uses besides this success-use. Sir Russell mentions one of them himself in his discussion of hallucinations. He also appears not to have noticed the 'dialectical' character of the remarks which he quotes from Professor Ayer's *Problem of Knowledge* on page 27. Here Professor Ayer is carefully weighing the pro's and con's of the sense-datum terminology. But after doing so, he proceeds to recommend that terminology—not to reject it—and uses it himself in the rest of the book. On the other hand, Sir Russell's reply to what some contemporary philosophers say about hallucinations is both trenchant and instructive, as we should expect from a writer who is an expert both on the neurology and the phenomenology of hallucination; and he justly accuses these philosophers of not knowing enough about the facts which they are undertaking to analyse. There is a very interesting account of some of these facts in chapter I (pp. 10-23) with detailed illustrations from a number of actual case-histories.

Finally, a few words should be said about the other 'modes of apprehension', the aesthetic and the religious. These are discussed in chapter III ('Symbol and Image'), though the last four pages of chapter II serve as a kind of introduction. The aesthetic theory stated in chapter III is greatly influenced by Collingwood's *Principles of Art*. Substantially, it is the Crocean theory that art is the expression of feelings. It is argued that we shall understand this theory more easily if we bear in mind the distinction already drawn between physical objects and perceptual objects. 'The physical object which is a work of art is one thing and the feelings which it evokes are another. But this is not true of the world of perception. . . . In the perceptual world feelings can be embodied in concrete form' (p. 49, *my italics*). I take this to mean that feelings, as well as sense-data, are sometimes actually constituents of perceptual objects. Thus, 'The artist when creating a work of art in his perceptual world, constructs it of his own feelings as well as his own visual, tactile, or auditory sense-data'. He does this by physically modifying a physical object, e.g. chipping a piece of stone,

or putting physical pigments on physical canvas. 'This physical object is then available to form perceptual representations in the perceptual worlds of other observers, when (if all goes well) they will find similar feelings of their own embodied' (p. 49). Sir Russell does not tell us how his theory would apply to the aesthetic properties of natural objects, though it is very obvious from several passages in the book that he is himself keenly sensitive to natural beauty. Presumably the spectator is here his own artist, so to speak. Unconsciously, without the physical sweat and toil of carving or paint-making, he just constructs a perceptual object for himself out of his own sense-data and his own feelings, and finds satisfaction in this perceptual embodiment of his feelings. The difficulty here is that, in some cases at least, the feelings appear to be themselves already aesthetic ones—for example, the delight which Sir Russell himself felt at the sight of the dolphins and flying fish, so felicitously described on page 2 of this book. When we are trying to give an analysis of aesthetic experience in terms of feelings, our analysis will be circular if the feelings are themselves aesthetic ones.

But although an aesthetic theory of this kind has some plausibility for the visual arts (including abstract painting and sculpture, pp. 52-54) and also perhaps for music (pp. 55-57) it does not seem to apply at all well to the verbal arts, especially poetry. I cannot see that it helps much to describe a poem as a 'verbal image' (p. 58)—any more than it helps to describe a picture as a 'statement' made in paint, which was a favourite expedient with art-critics at one time. The use of the word 'image' here suggests a failure to distinguish two very different senses of the word 'imagine'—imagining *that* so and so (by means of words or otherwise) and imaging, the production and contemplation of mental imagery. Mental imagery may no doubt 'embody' feelings, as sense-data or perceptual objects can. But it is by no means obvious how propositions entertained without belief, which are the objects or contents of imagining *that*, can be said to embody feelings. Perhaps they can, but it must be a very different sort of 'embodying'.

It seems to me that the word 'image' is equally overworked in the brief discussion of religious experience which follows. But the religious, or quasi-religious, attitude to the irremediable transiency of beautiful things, expressed on the last page of the book, is one which commands our interest and respect. Sir Russell finds comfort in the thought suggested by some very moving lines of Walter de la Mare, that the transiency of beautiful things is itself beautiful. I hesitate to cavil at this. But is it obvious that decay is always or even usually beautiful, even if other forms of transiency often are? And what of the 'inextinguishable hope' referred to on page 57, that time itself is 'only a mode of apprehension'? Should we do well to renounce this rather Kantian hope, if we could—though unfortunately we cannot, since it is inextinguishable?

H. H. PRICE

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Combination of Observations. By W. M. SMART.

Cambridge University Press, 1958. Pp. xiv + 253. 35s.

THE reduction and interpretation of statistical data have always been important in astronomy, and modern statistical theory owes much to early work on the theory of errors by such giants as Gauss and Laplace. The author of this book is Professor of Astronomy at Glasgow, and he has assembled a useful collection of illustrative examples which may help the young astronomer to see the need for statistical methods. Unfortunately, the book claims to be more than an exposition of such methods and to present general principles useful also, say, to research workers in microbiology or population statistics. The treatment of basic principles is too shoddy for this claim to be justified, and among numerous instances in the book which indicate this weakness the following list may be sufficient:

§ 1.11. The definition of density in terms of frequencies is dubious (the notation $F(x)$ for the density function is also unorthodox).

§ 2.09 *et seq.* The optimum accuracy of the arithmetic mean in an appropriately defined sense is never demonstrated.

§ 3.01. Two definitions of probability (the 'Laplacian' and the 'empirical') are given, but no discussion included of the relation of the empirical to the theoretical concept and of the law of large numbers. The lack of adequate theoretical formulation appears also elsewhere. (Cf. remark above on § 1.11; or refer to the discussion on page 47 on the unbiased estimate of the variance, put forward as an approximate formula. The effects of errors in the variance estimate on assessing the accuracy of the mean is never mentioned).

§§ 7.02 and 7.03. Polynomials or Fourier series are fitted to frequency distributions by least squares without taking account of the dependence of the accuracy of any ordinate on its magnitude.

§ 8.04. Moment and cumulant generating-functions are never used in the book, but the correction of an observed frequency distribution affected by observational errors can most simply be presented by means of them, as in effect the author discovers in § 8.11.

M. S. BARTLETT

On Mathematics and Mathematicians (Memorabilia Mathematica).

By R. E. MORITZ.

Dover Publications, 1958. Pp. vii + 410. 16s.

ONE would not expect a teacher's handbook of quotations to have much relevance to the philosophy of science, and this little book does not greatly exceed expectations. Yet it is worth a browse. The study of the 'logic of

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discovery' is becoming a respectable part of the philosophy of physical science, and it is likely that the existing literature on this aspect of mathematics will be enriched. Non-mathematicians whose conception of mathematics derives from Russell's unfortunate quip, 'mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true', may find this volume a useful primer of re-education.

Many mathematicians are quite conscious of the element of craftsmanship in their work, and are sensitive to aesthetic aspects of an argument. The better quotes of Chapters VI and XI illustrate this. Stray comments on mathematics by philosophers are neatly listed and indexed. Who knows this one of Voltaire's?

We admit, in geometry, not only infinite magnitudes, that is to say, magnitudes greater than any assignable magnitude, but infinite magnitudes infinitely greater, the one than the other. This astonishes our dimension of brains, which is only about six inches long, five broad, and six in depth, in the largest heads.

It should be kept in mind that this collection was made nearly a half-century ago, and so lacks any discussion on the revolution in mathematical methods which was getting under way then. It also suffers from the anthologist's lack of deep knowledge of the history of mathematics and its bibliography. For instance, the collection's value in its own terms would have been enhanced if use had been made of Aubrey's 'Brief Lives'. A few quotes from that source would establish beyond all doubt that mathematicians are, after all, quite human.

J. R. RAVETZ

'*Science in a Tavern*': *Diversions on Science in the Making*. By Charles Slichter.

University of Wisconsin Press, 1958. \$1.00.

UNFORTUNATELY this intriguing title is limited to the first, and just possibly the second, of this series of articles and lectures. Taverns are mentioned only incidentally in the later chapters and some of the essays are in no way concerned with science. This is a disappointment. Science is public information and it would have been interesting to have followed how, during the course of centuries, scientific ideas were developed in the Public House. Indeed, the first essay on 'The Royal Philosophers', though it deals with only one group of scientists, is by far the most interesting part of the book.

Your reviewer, having recently partaken of refreshment in the course of several all-day scientific meetings in this country (not, of course, of the Royal Society!), has sometimes asked himself and his colleagues why the English scientists of today tolerate such scanty and poor fare. Our ancestors had no

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such tradition. Professor Slichter quotes several menus of the Royal Society Dining Club; here is one (1747): two dishes fresh salmon, lobster sauce; cod's head, pidgeon pie, calve's head, bacon and greens, fillet of veal, chine of pork, plum pudding, apple custard, butter and cheese. The drinks (except for claret) were not listed but a French visitor leaves us a record that they were not lacking. In one respect only, conditions may not have changed so much. The Frenchman writes: 'In France, we commonly drink only one cup of good coffee after dinner; in England, they drink five or six of the most detestable kind.'

Membership of the Royal Philosophers' Dining Club was, with one exception, limited to men of great distinction; yet, by 1750, the exception was made that any gentleman presenting the company with an adequate haunch of venison might be admitted. Soon afterwards, it was ruled that a sea turtle might be substituted for the venison and thereafter the claims of other delicacies had to be met. As the author puts it: 'In modern times, the newspapers would probably refer to this as the "honorary membership graft".'

The later essays are much less interesting. Written at various dates during a long academic career, many of them are of ephemeral significance. Moreover, in so far as they refer to science or philosophy they contain nothing that is new. To mark the division of the paragraphs, the publisher has inserted a strange device which, on close inspection, is seen to be a slightly distorted surface-integral sign. This seems to reflect all too well that the matters discussed are summarised at a very superficial level.

Even if the subject matter had been significant, it would have been wiser to write a new book incorporating a lifetime of ideas, rather than merely to reproduce, with hardly any editing, a series of lectures and articles written over so long a span of years.

But the whole book costs only a dollar; perhaps it is worth this for the fun of the first chapter.

G. W. SCOTT BLAIR

Les Limitations internes des formalismes. By Jean Ladrière.
Gauthier-Villars, Paris, 1957. Pp. xiii + 715.

THIS is an encyclopaedic survey of Gödel's Theorem and related results on the undecidability of mathematical statements and the incompleteness of formal systems. Detailed accounts are given of Gödel's original paper (1931), the theorems of Church, Kleene and Rosser, Tarski's semantical researches, and related work by Chwistek, Curry, Henkin, Kalmár, Kreisel, Wang, and many others. The author is plainly a man of unusual patience,

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willing to go to extraordinary lengths to document his exposition: 46 pages are needed for the bibliography, 42 for a table of symbols, while indexes and similar materials occupy a further 127 pages.

The book is intended for readers with no previous knowledge of mathematical logic (p. ix): its purpose is to provide them with sufficient understanding of the relevant mathematical arguments and conclusions to permit profitable discussion of relevant philosophical issues. I doubt whether the purpose has been achieved, in spite of the author's impressive mastery of this difficult subject. His accurate and well arranged reports remain close to the original versions, with only minor changes in symbolism and in order of presentation. Consequently, little is done to help the beginner to overcome the serious obstacles to comprehension that await him. To take an example: Ladrière says several times that Gödel's undecidable statements assert their own unprovability ('*Il s'agit d'une proposition qui affirme sa propre inderivabilité,*' p. 93). Although Gödel himself spoke in this way and has been followed by Mostowski and many others, the assertion is misleading unless carefully glossed. For beginners it might be wiser to stress that the undecidable statements belong to arithmetic and are about the integers—and so to avoid troublesome qualms about circularity and self-reference. The correct view is well explained in Rosser's informal exposition, with which Ladrière is of course well acquainted. Again, no informal exposition known to me succeeds in making plain (for the hypothetical novice) the mathematical *Gestalt* of Gödel's climactic arguments contained in the few pages where he finally *proves* the decisive conclusions. Here, too, Ladrière is a faithful reporter: we see the trees all right, but the wood somehow remains obscure.

The tentative philosophical conclusions advanced by Ladrière are disappointing. It is not illuminating to be told that the meanings conveyed by mathematical language are rooted in an experience *sui generis* by which we have access to the domain of mathematical reality (p. 406) or that mathematical existents are not exhausted in their manifestations (p. 443). But then the philosophy of mathematics makes the best minds begin to stutter.

MAX BLACK

Man and Time: Papers from the Eranos Yearbooks, Volume 3. Edited by Joseph Campbell.

Routledge and Kegan Paul, London, 1958. Pp. xx + 414. 35s.

SINCE 1933, 'Eranos' conferences have taken place nearly every year, at the home of Frau Olga Froebe-Kapteyn near Ascona, on what might perhaps be termed esotericism and exotericism in the East and in the West. The

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lectures have been published in a series of *Eranos-Jahrbücher*. The present volume is the third of a series of selections in English translation.

The earlier volumes, 'Spirit and Nature' (1945) and 'The Mysteries' (1955) were more fortunate in having reasonably univocal themes. 'Time' is a word in many different types of language, and this collection of papers presents a confusion of tongues as well as a confusion of referents:

'Our discussion will deal principally with the relation of art to its epoch; the second part of our lecture will take up the specific relation of modern art to our own time.' (E. Neumann, 'Art and Time').

'This and this alone will enable us to observe that despite appearances, despite the almost total absence of explicit testimony, the problem of time lies at the very heart of Gnosis.' (H.-C. Puech, 'Gnosis and Time').

'What is certain is that in progressively decelerating the rhythm of breathing in prolonging the expiration and inspiration, and increasing the interval between these two elements of respiration, the yogi experiences a time different from ours'. (M. Eliade, 'Time and Eternity in Indian thought').

It would be interesting to know how successful was the discussion at this conference (that of 1951) in creating a metalanguage for its purpose. The editor merely remarks 'Our minds at this meeting were prodigiously churned'. An introductory essay, 'The Time of Eranos', by Henry Corbin, is poetic rather than exegetic in this respect.

A paper by Professor Max Knoll, 'Transformations of Science in Our Age', may be selected for comment here. It is in two, not very closely related, parts. The first, 'Growing awareness of typical pairs of aspects in physics, psychology, and other sciences', seeks meaningful analogies between wave/particle, conscious/unconscious, intuitive/observational, form/process, essence/existence and so on; the rewards of this quest decrease to triviality as its successes mount up. The second part, 'Astrobiological, astrophysical, and physical time', assembles some interesting curiosa: sunspot frequency and tree growth; metre waves and cell division in plants; planetary configurations and radio disturbances; 'correlations between the 27-day period of the sun, the frequency of magnetic storms and of northern lights, the death rate, and the rate of incidence of [deaths from] certain diseases'; atmospheric and pigeon orientation; magnetic-storm cycle and birth-dates of Nobel Prize winners. I am not sneering. I am all for a professor of electrical engineering, like Dr Knoll, daring to ride a hobby-horse outside his own field, ignoring the tracks beaten by others, to keep alive a sense of wonder, curiosity, speculation ('cavalier/roundhead'!).

One is more inclined gently to mock a specialist's naïveté in his professed field, as in the third of the passages quoted above. In sleep, in states of excitement, pain, fear, suspense, we all have different experiences of time; whatever there is of interest in mystical experience, it is certainly not this.

G. E. DENYER

The Freedom of the Will. By Austin Farrer.

A. & C. Black, London, 1958. Pp. xi + 315. 28s.

WOULD that all theologians had as light a touch and as lucid a style as the author of these Gifford lectures! Here is philosophy at its wittiest, well-informed, yet free of bookishness almost to a fault. Nothing here to repel needlessly the restive scientist, anxious to know whether his research on the brain leaves other people's will free.

What will he learn? Certainly, if he is fair, a new sympathy for old-fashioned ways of posing questions. Dr Farrer has no inhibitions from enquiring after such things as the 'seat of consciousness' (as long as we distinguish the psychological from the physiological seat); and when he is done, the reader may see more sense in it than current fashion had allowed. Throughout the book he may have to read rather carefully, for the author delights in protracted (and pretty convincing) advocacy of opposing views in turn, so that random sampling becomes both confusing and dangerous. But on all the usual topics of mind and body, motive and choice, responsibility and freedom, he will find an immensely enriching and stimulating fund of epigram and imagery, and some good arguments.

Naturally, he will be eager to know what such an attractive thinker makes of the causation of bodily action; but it is just here, despite some really excellent ground-clearing, that the scientist may find himself drifting out of contact with yet another metaphysician: which is exasperating, for this one has been fun to follow. Epiphenomenalism is untenable: Good. Parallelism is untenable: Good. Physical laws are not disobeyed by brain-mechanisms: fine. But in the brain 'The minute elements function regularly, yet allow themselves to be overruled . . . (p. 52) . . . the (biological) pattern of action does real work . . . and leads (them) a dance they would not otherwise tread' (p. 60) ('otherwise' in what sense?). And then (p. 99) '(this) "bewitching" of our minute physical energies by organising patterns remove(s) their operations from the sway of physical exactitude'.

Alas—where are we now? Just when the author (having dismissed the Cartesianism of Professor Eccles) seemed ready to grasp the nettle and declare that human agency is personal agency (and free at that) whether or not the human brain is physically (exactly) determined in its actions, he introduces *ad hoc* a concept that effectively makes free personal agency subject, as in so many other arguments, to our ignorance of physical causation.

That such ignorance is in practice inevitable we need not dispute. Our question must only be whether it is *necessary*—whether it is needed to make room of the right sort—for the validation of freedom and responsibility, (particularly as conceived by biblical Christian theology). Is not what is minimally necessary reducible to something much weaker—namely, the *logical* indeterminateness, *for the agent*, of the choice he must make? It can

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be argued (as I have recently argued, *Mind*, 1960, 69, 31-40) that such indeterminateness is inevitable even if to the isolated *onlooker* the agent's brain enjoyed no release from 'the sway of physical exactitude'. Until the implications of this indeterminacy have been exhausted, it seems a pity to postulate (without apparent justification) anything more queer and scientifically frustrating. Before we can either assert or deny that 'the occasion determines the form' of a change of mind (p. 165) we must ask 'for whom—for the agent, or for the onlooker?' Determinateness in such a case denotes not a unique *status* of an event, but a *relation* between the event and its contemplator, which is demonstrably not the same for the agent as for the onlooker.

Despite the force that is lost from some of Farrer's arguments when we thus sort out the beliefs that could be valid for onlooker and agent respectively, he makes many points of great value. To cite only one example, it is refreshing on page 269 to find a robust rejection of the common tactic of admitting most decisions to be determined (physically or psychologically), but clinging to the hope that a residuum of sufficiently important choices are exempt. As he rightly sees, normal deciding is not a matter of residual free play in the bearings of an otherwise determinate mechanism: 'Decision as such, any decision, is the foundation of a sound libertarianism. . . . Any libertarian edifice which narrows its base, and builds on privileged decisions . . . is doomed to fall. For once we admit that most decisions are determined, we cannot resist the contention that all may be.'

By contrast, his discussion of the power(lessness) of 'machines' to 'exceed their principles of construction' (p. 286) shows curiously little grasp of current thinking on self-organising and self-reorganising systems. To many of us the limits here seem in principle no more restrictive than those set by human genetic machinery on the capabilities of human beings. If space for more adequate discussion was lacking, this whole topic might have been better omitted; for it is one with which any realistic metaphysics of personality must in the end come earnestly to grips.

An intricate work of art such as this cannot escape misrepresentation in a brief review. The reader is urged to enjoy—and assuredly to profit from—the experience of its direct impact.

D. M. MACKAY

Inside the Great Mirror. By James K. Feibleman.
The Hague, 1958. Pp. 216. Guilders 19.

RUSSELL's new logic raised anew the problem of realism versus nominalism. Russell himself attempted to develop a new nominalist theory of language to fit this new logic. This attempt was unsuccessful. Next came Wittgenstein's attempt, which was also unsuccessful. The theory which this attempt resulted in (the *Tractatus*) is by now almost entirely abandoned;

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only one of its corollaries survives, namely that metaphysics is meaningless. It survives, although there is no reason to support it, because of its usefulness for the obscurantist.

This seems to be the crux of Professor Feibleman's story. While narrating it he makes many very interesting remarks on many sidelines or detailed problems. In many respects this book is very good indeed, but I can recommend it only to the reader who can discriminate between the chaff and the grain.

The historian of contemporary philosophy will delight in Feibleman's interpretation of Wittgenstein's *Tractatus* because he takes it very seriously and sees in it an attempt to solve one of the philosophical problems of that period. It is a pity perhaps that Feibleman discusses only one issue of that period and no others; I particularly missed Russell's paradox; but as far as it goes the analysis is good. Readers of this *Journal* will be interested in Feibleman's interpretation more than in others as it is related to problems of the philosophy of science. Anti-metaphysicists will probably hate his jokes and pro-metaphysicists may enjoy them, at least when they are made in good humour. Both parties will be puzzled by his coupling of leniency towards the old Wittgenstein with harshness towards his closest disciples. Feibleman's claim that 'a follower is small just in virtue of being a follower, since he is not a leader' (p. 152) is unjust. With the possible exception of Wittgenstein and his like, we are all humble followers to some extent or another (Feibleman himself is a professed follower of Peirce and of Whitehead) and lack of originality is no sin. I hope that readers will pay attention to the valuable core of this book and ignore its weaker parts.

J. AGASSI

Sherrington: Physiologist, Philosopher and Poet. The Sherrington Lectures, IV.
By Lord Cohen of Birkenhead.

Liverpool University Press, 1958. Pp. xiv + 108. 17s. 6d.

THIS work in an excellent exposition, mainly factual, but with some perceptive philosophical criticism, of Sherrington's career, and his extraordinarily large number of works on many different subjects (the majority physiological). The section on his philosophy occupies only about 13 pages of a total of 108 pages (which include 35 pages of bibliography), and ends with the admission that in the field of the relationship between mind and body, 'Sherrington's thinking cap was second-hand—it had been worn by all the exponents of dualism for three centuries since the days of René Descartes' (p. 64).

Sherrington in his experimental physiology (for which he is best known and with most reason ranked as a genius) was concerned mainly with

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reflex activity, for, he said, 'it can be studied free from complication with the psyche'. He thought that some parts of the brain 'may be called mental', and that therein 'nerve-actions exist still unknown to us, and that these may correlate with mind. . . . There is, so far as I know, in the chemical, physical properties, or microscopical structure no hint of any *fundamental* difference between non-mental and mental regions of the brain.' Such confusion of use of the words 'non-mental' and 'mental' will invalidate, for many modern readers, other statements about brain and mind, neurology and psychology.

As Lord Cohen expertly points out, Sherrington was aware of the semantic problem attached to such words as 'life' and 'mind', since he discusses them, in part, in relation to the term 'life', 'yet he surrendered to the doctrine of body-mind dualism, and then, failing to unify its disparate entities, re-enunciated its philosophy'.

How curious it is that the neurologist and neurophysiologist, who are, by their work and interest at the junctional point of science and philosophy, in the ideal position to make the grand synthesis, should be so afraid of or disinterested in and insensitive to their opportunity and obligation, that they remain within the narrow field of reflex activity and clinical description. So the philosophy of even the greatest neurophysiologist is justly estimated as 'second-hand'; and neurology contracts like a withered bud, to the dismay of those who perceive its unique position in the search for knowledge and wisdom.

WILLIAM GOODY

Religion and the Scientific Outlook. By T. R. Miles.
Allen and Unwin, London, 1959. Pp. 224. 21s.

MR MILES maintains: (1) That sentences in which the word 'God' occurs have no literal factual meaning. (2) That consequently they cannot logically conflict with any scientific statement or set of statements. (3) That the most important use of theistic language is the telling of parables, and the appropriate religious policy is one of silence qualified by parables.

Three features of parables are mentioned: (1) They contain empirical assertions. (2) For the parabolic purpose, it is unimportant whether these assertions are literally true. (3) Parables carry a message and may call attention to timeless truths.

The logical crux of this kind of analysis concerns the relation between parable and parabolised, between the story and its interpretation. The logic of 'parable', used in this way, appears to imply the possibility of formulating a non-parabolic, literal assertion which expresses the parabolic message. If so, reasons should be given why such reformulations of the content of the

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parable are logically incapable of conflicting with scientific statements. In a discussion of the Christian parable of the Creation in this book, the interpretation takes the form of different theistic assertions, a piling of parable on parable. To appeal here to the necessity for religious silence would not meet the point, since the question is whether one has the logical right to speak of parables in contexts where one does not know how to speak non-parabolically.

This lack of clarity about the logic of 'parable' seems to me to invalidate the parabolist's answer to positivist criticism of religious utterances. But in any case, the thesis of the logical independence of scientific and religious assertions is appreciably weakened by the author's candid admission that scientific discoveries, though they cannot formally contradict the parables of religion, may nevertheless make these appear 'implausible' or 'inappropriate'.

R. J. SPILSBURY

What is Cybernetics? By G. T. Guilbaud.

Heinemann, London, 1959. Pp. 126. 10s. 6d.

ONE answer of M. Guilbaud's is that Cybernetics is no super-science but an inter-scientific instrument of analysis, an ideal telephone exchange linking up specialisms (with the insinuation that in the bad old pre-1940 days specialists did not get together over problems of common interest). Another, different answer developed in this essay is that Cybernetics is, or may become, part of a general theory of human action and decisions. This uncertainty is ascribed to the fact that Cybernetics lacks, as yet, any unifying principle to give it coherence.

M. Guilbaud's exposition is restrained and readable, and shows awareness of the historical roots of this not very novel phenomenon.

R. J. SPILSBURY

ANNUAL CONFERENCE OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

THE fourth annual conference of the British Society for the Philosophy of Science was held from 25th to 27th September 1959 at Newnham College, Cambridge, and was attended by about eighty members and guests. Dr M. B. Hesse was conference secretary.

Four symposia were held: 'Scientific Research and the Philosophy of Science', 'Biology and Physics', 'Classification, Concept-formation and Language', and 'Knowing and Being'.

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The first session (Dr J. O. Wisdom in the chair) argued whether 'philosophy of science' can be held to refer to any activity not properly subsumed under 'scientific research', and, if it can, whether that activity is relevant to research. Professor H. Dingle mentioned various questions of value and purpose, as well as of method, papers on which were unlikely to be accepted for publication in journals concerned with particular sciences; he suggested that many research workers had something in common with people hacking passages in a thicket, pausing only to exhort others to join them, and to shrug at those who asked where they were going, and why, and whether their tools were capable of improvement. Professor H. C. Longuet-Higgins, on the other hand, argued that of the products of philosophers of science, part was science (and, as such, *was* the concern of scientists), part was philosophy (of concern to man generally), and the remainder was of no use to man or scientist. This aroused some consternation, as members consulted their own credentials and invoked those of others, but in due course a consensus emerged that scientists have to think in some sense philosophically and the comparative study of modes of scientific thought may help them to do so. The conference did continue.

In the second session, Professor J. H. Woodger described an abstractive hierarchy of terms characterised by a one-many relation, and its use as a conceptual framework in biology. With some hierarchies of cells, every cell was a distinct life; with others only the first cell in each hierarchy, elaboration occurring on subsequent levels. Morphology was the study of the arrangement and differentiation of parts; physiology was the study of the existential dependence of parts. Genetics was concerned with the process in which the first member of a hierarchy results from the conjunction of two members of other hierarchies. Dr E. H. Hutten set alongside this framework the sequence exhibited by radio-active decay: each new atom produced was similarly time-extended, stemmed from a parent atom, and consisted of existentially dependent parts, but it was not dependent on its environment analogously to a cell. In elementary-particle theory, explanation might amount to little more than classification, or an enumeration of possibilities, and might have little more predictive power than comparable biological models with their over-determination and multiple-causation. The chairman (Professor C. F. A. Pantin) referred to the occurrence, in biology, of morphological models that are interpretable at more than one level in an organisational hierarchy. In discussion it was suggested that the predictive power of a theory might not always be manifest at the time of its original formulation; it might have to await development of deduction. There was much interest in the relative importance of the past history of an entity in biology and in physics: *historical* existential dependence appears to be a function of complexity.

In the third session (Mr G. Buchdahl in the chair), Miss M. Masterman and Mr R. M. Needham presented the strategy, and some of the tactics, of a method of analysing language by assimilating it to a library classification system in which concepts are arranged on a finite lattice ordered by a single, weak, 'concordance' (inclusion) relation. They further suggested that the formation of scientific concepts is a development of language according to this model. There was some discussion as to whether the method is a technology for mechanical translation, or a science, or a philosophy of language, and it was suggested that it could be viewed as a scientific model of language, containing the partly uninterpreted concept 'inclusion', and capable of being tested by experiments on translation and on analogy-finding.

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In the fourth session (Dr M. H. Thorpe in the chair), Professor M. Polanyi presented a way of talking about the primary process of knowing by perception—the pre-articulate act of knowing, which partakes of the uniqueness of the individual percipient, the unspecifiable personal knowledge from which any specifiable, potentially public, knowledge is derived by a process of abstraction. Complex entities were commonly perceived and recognised as wholes before particulars had been identified; the process of discovery, in fact, might be regarded as an alteration of analysis, recognising particulars, and integration, recognising the relations of parts to the whole. Professor Braithwaite suggested that too narrow a view might be taken of specifiability, and that some levels of subjective experience, however vague, could be conveyed by language—for example, 'Oblomov' conveys the experience of laziness; beyond that, he differed from Professor Polanyi over his use of the term 'knowledge' for what was unspecifiable. In the subsequent discussion there was some reluctance to focus on this rather undemocratic mode of tacit awareness, and a preference for talking about what *can* be made public—with the implication that the progressive refinement of scientific language tends to eliminate the unspecifiable.

In conclusion, it is perhaps worth drawing attention to the unusualness of a scientific conference at which speakers are not armed with specified and verifiable data but attend primarily to make as explicit as possible how they think, and to receive criticism of the process thereby revealed—especially from those who are not working in the same field. The coherence of this universe of discourse was illustrated by the frequency of reference from one discussion to another, the *esprit d'escalier* from one session often finding its outlet in a later one; its range, by the frequency of spontaneous quotation, not only from Shakespeare and Wordsworth but also from Swinburne (and *early* Swinburne, at that).

G. E. DENYER

ERRATA

In the November 1959 number, p. 256, line 5, for 'lustre' read 'lustra'.

In the paper 'Probabilistic Independence and Corroboration by Empirical Tests' by K. R., Popper, this *Journal*, 1960, 10, 315-318, the following three corrections should be made:

p. 315. insert, on the left hand side of the table of possibilities at the bottom of the page, ' $\omega =$ ' before ' $\sim e$ ', so that ' $\omega = e$ ' stands before the lowest bracket.

p. 315, add to the end of line 5 before the paragraph numbered 4, the following footnote 2:—

2. This shows that it is not enough for Professor Leblanc's purpose to outlaw absolute probabilities. What we should have to outlaw is the probabilistic theory of the intuitive idea of *independence*—a theory which is of the utmost importance for most applications of probability.

p. 317, line 5. ' $-e = \omega$ ' should read ' $\sim e = \omega$ '.